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I. NIAGARA.

GLACIAL AND POST-GLACIAL PHENOMENA.

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THE glacial phenomena of the district of Niagara have been so often described, and the cause of, and the time occupied in, the excavation of the river gorge, so often discussed, that I did not expect, when, on Christmas-day, I made my first visit to the great falls, to have anything new to record, but went quite prepared to acquiesce in the conclusion that has been received for more than thirty years, that the whole of the gorge, from Queenstown to the falls, has been excavated since the glacial period. Since this theory was first advanced, many geologists have visited the district, and, so far as I can learn, no one has called in question this verdict; it has been accepted as an established fact, and various calculations of the time necessary to excavate the gorge have been made, throwing back the occurrence of the glacial period from 30,000 to 300,000 years ago.

It was with great surprise, therefore, that I found, that at first sight this conclusion was not evident, and that, on further examination, it was not tenable. I feel that in having to oppose the theory that the gorge of Niagara has been excavated since the glacial period, I shall be adding another scientific heresy to the many that are recorded against me; but the heresies of to-day are the truths of to-morrow, and I shall at least give my reasons for believing that my explanation of the problem ought to be classed in the latter, and not in the former category.

The question of the excavation of the gorge cannot be clearly understood without some knowledge of the glacial deposits, and I shall in the first place describe the glacial, and afterwards the post-glacial phenomena. So many authors have written on the subject, that I shall only mention those from whose works I have obtained information of importance. Foremost in the list stand the names of Sir Charles Lyell and Professor James Hall, who visited the district together in 1841, and who afterwards published the conclusions, that they appear to have arrived at together. Sir Charles Lyell, in the "Proceedings of the Geological Society of London" for 1842 and 1843, and more fully in his "Travels in North America," where there is an excellent coloured bird's-eye view of the falls of Niagara and adjacent country, and also a geological map of the district, in which the reader who has not visited Niagara may correct the false impression he is likely to obtain, from the necessary foreshortening in the bird's-eye view, of the small distance between the falls and the whirlpool, which are, in reality, four miles apart. Professor James Hall published nearly identical opinions in the "Boston Journal of Natural History" for 1843-44, and more fully in the "Geology of New York," Part IV., in 1843. The latter work contains not only a bird's-eye view of the district, but an excellent map of the falls, constructed from a trigonometrical survey made in 1841, by Mr. Bakewell: afterwards in 1842, corrected by Professor Hall and two engineers. The whole of Professor Hall's observations on the glacial phenomena of the State of New York should be read by those interested in the study of the glacial period. They abound in original remarks, and in clear descriptions of the succession of the superficial deposits, and many of the conclusions at which this eminent state geologist arrived more than thirty years ago are only now receiving in England the attention they deserve. Professor Hall also describes other rivers running into Lake Ontario from the south, which, like Niagara, have had their pre-glacial channels filled up, and have since taken a more westerly course to the lake.

In 1859, Professor Ramsay published, in the "Quarterly Journal of the Geological Society," his observations on the glacial phenomena of Canada, made during a trip to that country in the preceding year. In this memoir he pointed out, I believe for the first time, that the river must have commenced to cut back the gorge at Queenstown, before the close of the glacial period.

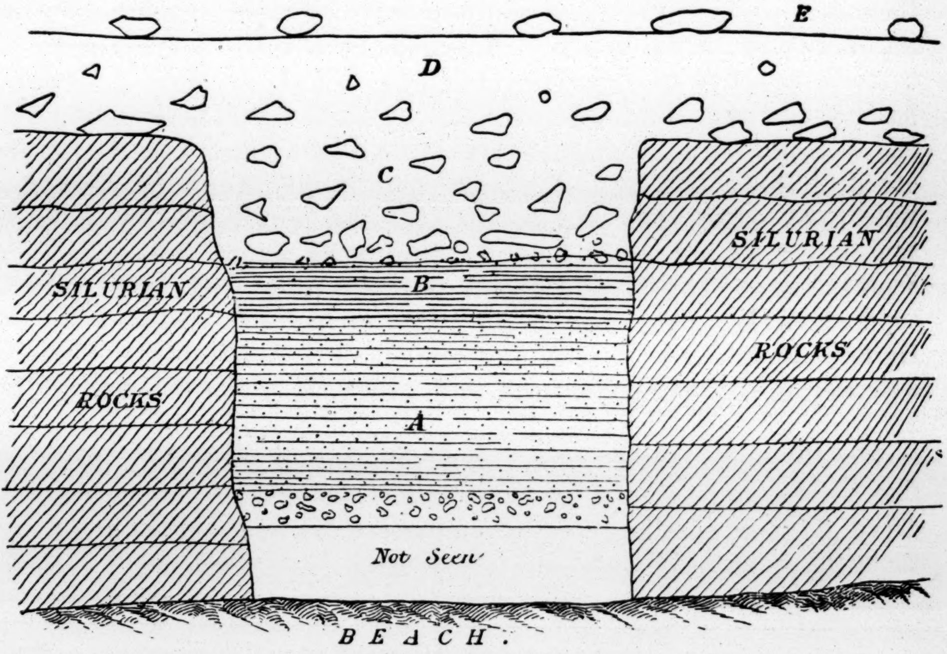
To Dr. Newberry, the accomplished chief of the Geological Survey of Ohio, I am greatly indebted, not only for much personal kindness and assistance, but for an early copy of his "Surface Geology," to be published in the forthcoming volume on the "Geology of Ohio," from which I have obtained a vast amount of information respecting the glacial deposits of the district of the great lakes. A very large amount of information is contained in the well-known works of Prof. Dana, not only in his admirable "Manual of Geology," but in various memoirs, amongst which I may especially mention his "Geology of the Newhaven Region," published in the "Transactions of the Connecticut Academy" in 1870. I may mention, that with the exception of Dr. Dawson, of Montreal, the whole of the most eminent of the geologists of eastern North America are now agreed that the principal glaciation of America was effected by land-ice, though there is abundant proofs, as I shall have occasion to show in this paper, that at a later stage, boulders were scattered over the country by floating icebergs. That later stage of floating ice was due, however, I contend, both in America and Europe, not to a submergence of the land below the ocean, but to the production of immense lakes of fresh water, by the damming up of the drainage of the continents by ice that flowed principally down the ocean depressions. In this conclusion, I have as yet no supporters amongst the geologists, either of America or Europe, if, indeed, I may not except Professor Hall, who informed me, in conversation, and authorised me to publish his opinion that the sea has never encroached on south-eastern New England since the deposition of the "*till*," and that the terraces of the Hudson and Connecticut were produced by the blockage of their waters by ice that flowed down the ocean bed, and of the presence of which we have proof in the immense moraines that compose the whole of Cape Cod.

GLACIAL PHENOMENA.

The rocks through which the gorge of Niagara is cut are limestones, sandstones, and shales. These rocks are all rounded and smoothed, and the limestones are frequently scratched and grooved. Besides the coarser ice marking, the rounded and smoothed surfaces of rock, when examined closely, exhibit innumerable fine scratches, which have been ascribed by Hall to small particles of sand imbedded in the ice that moulded the rocks, and he has shown the improbability that this moulding and fine scratching, which is universal over the whole northern part of the State of New York, wherever the rocks are of sufficient hardness to receive and retain striæ, could have been effected by icebergs. Lying on these glaciated rocks are superficial deposits of drift, containing beds of unstratified clay, with boulders, sands, and loam. These are spread over the whole district like a mantle, so that natural exposures of the bed rock are rare, excepting in the gorges cut by the river.

The cliffs bordering the gorge, from the falls to Queenstown, are everywhere capped by these deposits; one of the most interesting and instructive sections of which is exposed at the whirlpool, four miles below the falls, at the end of the filled-up pre-glacial gorge that runs down to St. David's. The following section exhibits the succession of deposits that fill the old gorge:—

Fig. 1.



Section through the old gorge at the whirlpool, along the line A B in Plan, Fig. 3.

The lowest bed seen by me at the section is that marked A, which consists of clean yellow river sand, with occasional seams and rolled lumps of clay. Below these sands there were exposed, when Lyell described the beds,* strata of pebbles, cemented together by carbonate of lime, overlying laminated clays. I saw one large mass of the pebbly conglomerates lying on the beach, and have shown its position in the section underlying A, but I have not inserted the laminated beds mentioned by Lyell, as some that I saw low down in the gorge had evidently slipped down from A and B, the whole face of the unconsolidated materials filling the gorge, showing many slips produced by rain and frost.

The bed of laminated sands (A in section) graduates upwards into fine laminated silt (B in section), the powder of which is almost impalpable when rubbed between the fingers. Higher up this silt is unlaminated, and contains a few small angular stones. It gradually changes upwards into C, which is a true "*till,*" or "*grund morane,*" containing large angular and subangular stones, many of great size, scratched and grooved. All the blocks imbedded in the clay are of the local limestones. A few rounded boulders, of northern origin, lie on the surface slope, but they have evidently rolled down from above. The higher part of C contains fewer and fewer stones, until it merges into D, which is composed of unstratified clay or "*till,*" without stones. Of this there is from twenty to thirty feet in thickness, the upper part being more sandy than the lower, and sometimes obscurely stratified. On the surface are a few rounded boulders of granite or gneiss, all far travelled from the north, and it is noticeable, that whilst the angular blocks in the "till" are all of local origin, those lying on the surface are almost, if not quite, always of distant derivation, and are invariably rounded or subangular. Those seen on the surface, near the whirlpool, were all of granite or gneiss. The continuation of the till (C and D in section) is shown in some small valleys that run into the gorge at the whirlpool.

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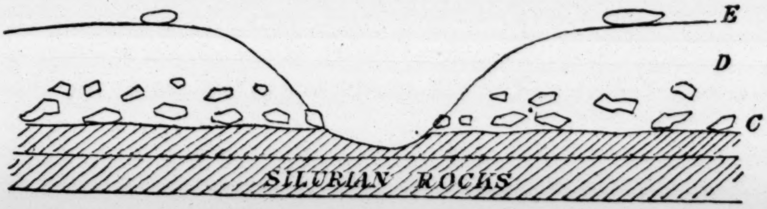
* Travels in North America, vol. ii. p. 95.

Fig 2.



Section of small valley running down to the gorge at the whirlpool. C, till with stones; D, till without stones; E, rounded boulders of northern origin on surface.

Here the till (C and D in section) lies upon the rounded and smoothed surface of the limestone. Some of the blocks of limestone at this point are of great size; one I measured was 9 feet by 6 feet, its thickness not seen, as it was half buried in the ground. On the opposite side of the river, and about a mile distant from it, I saw exactly the same succession of beds exposed in the cutting of the railway, half way between the railway bridge and Lewiston. The northern end of the old filled-up gorge at St. David's shows a similar succession of beds, with the addition, that on mounting the plateau from the lower one of the lake of Ontario, I found, exposed in railway cuttings, that the till without stones was capped by stratified beds of clay and sand, with a few lines of small pebbles.

The above sections may be taken as typical ones of the superficial beds that mantle the whole of the northern part of the States of New York and Ohio, and much of Canada, and I proceed to show that they are exactly what would be produced by the accumulation of a great mass of ice in the north, that gradually progressed southward, and that afterwards melted back again as gradually as it had advanced.

Let us carry ourselves, in imagination, back to the preglacial times, when the old river ran through the filled-up gorge from the whirlpool to St. David's, and try to follow the successive steps by which it was filled up, and ultimately completely obliterated, or rather concealed. Let us bear in mind that the Niagara runs northward, in the direction from whence the ice came. Hall, Dana, Newberry, Lyell, and Ramsay, have all pointed out both from the scratchings of the rocks and from the transported blocks in the till, that the movement of the ice was from the north. It has also been clearly shown that the ice flowed up the St. Lawrence valley, from the north-east. It advanced up the slope of that great valley principally by the overflow of the higher parts of the ice over the lower. That there was some movement of the lower part of the ice from the pressure from the north is, however, sufficiently proved by the different formations that crop out from east to west, having furnished stones to the till that covers the rocks immediately to the south of them. Thus, according to Hall, huge blocks of Medina sandstone are moved southward unto the top of the Niagara limestone. In like manner, numerous masses of the Niagara limestone are drifted forward unto the Onondago salt group, and still further south, on the Chemung limestone, lie great numbers of immense blocks from the Onondago salt group to the north. The size of these fragments bears a proportion to the distance they have been transported from the parent block, the largest being nearest to it. This is characteristic only of the till, and not to the northern boulders that are strewn over the surface, and which have not been transported from their distant northern homes by land ice.

The immediate effect of the ice, as soon as it had dammed up the mouth of the valley of the St. Lawrence, must have been to form a great fresh water lake in front of it, on which it was constantly advancing. When, after filling the basin of the Lake of Ontario, it had, in its progress south-westward, reached the base of the cliffs of the Queenstown escarpment, or had dammed back the water to that level, the commencement of the filling up of the old Niagara gorge was at hand, and from that time, during the advance of the ice and its subsequent retreat, the deposits of sand, till, and boulders shown in the sections were made. The first step was the partial arrest of the flow of the old river, causing it to deposit at a higher level than its original bed, first pebbles forming the conglomerate at the base of the section in fig. 1, and then the thick bed of river sand, when the current was still more impeded. The bed of fine silt (B in fig. 1) marks the time when the flow of water to the north-east was completely stopped. Dr. Newberry, several years ago, first drew attention to the fact, that at some time during the glacial period, all the great lakes of North America drained towards the Ohio and the Mississippi, and since then, several deep channels by which they did so have been described.

I did not see this fine bed of silt in any of the other sections I examined, and I think its preservation in the one at the whirlpool must have been entirely due to the protection the nearly perpendicular walls of the gorge afforded against the great pressure of the ice that passed over it. Its upper portion contains small angular stones, and it gradually merges into the unstratified till, containing large angular blocks.

It is probable that, during the advance of the ice, no till or *grund morane* was formed below it, but that the smoothing and scratching of the surfaces of the solid rocks were then effected, and that the till was deposited beneath the ice when it was melting back, and its pressure being gradually lessened. Mr. Bonney has objected to the theory of both the erosion of rock surfaces and the deposition of till having taken place below the ice.* But the two actions belong to different times; the one was accomplished during the advance, the other during the retreat of the ice. The effects are similar to those of a mountain torrent, which, when full, carries all before it, but which, when its waters lessen, deposits stones and mud in its course. During the advance of the ice, there could be little deposition below it, all the stones held at the bottom of the moving mass being probably ground to powder; but, as it melted back, the stones and clay held within it would be deposited at its foot.

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* **Nature, vol. x., p. 85.**

Dana, in an excellent paper on the Glacial Era in New England, has ably argued this question, and has shown the enormous power that moving ice, 6000 feet thick, with a pressure of at least 300,000 pounds to the square foot, would have in abrading the rock surfaces below it, and carrying forward in its lower part the loose material it had broken off or caught up from the rocks below, and how the whole of this would be deposited at the melting of the ice.* It would greatly conduce to clear descriptions of glacial phenomena, if the old term "till" were confined to this deposit. It is the "Erie clay" of Dr. Newberry, the "Lower Boulder clay" of Wood, the "*grund morane*" and the "*moraine profonde*" of others. "Erie clay" is a local name, and includes stratified beds of a different origin. "Boulder clay" is often a misnomer, as frequently this clay contains no boulders. "*Grund morane*" and "*moraine profonde*" indicate a particular mode of origin, which, though probably correct, is still theoretical. "Till" is an old English word, long applied to this deposit, and may be used by every one, whatever theory of origin they may favour. I suggest, therefore, that it should be confined to designate the unstratified clay with angular blocks, generally of local origin, that lies at the bottom of all the glacial beds, and that the term "boulder clay" should be applied to the higher beds, which show the action of water as well as of ice. The term "drift" might be applied to any glacial deposit the nature and origin of which is doubtful, in the same way as the name "trap" is used for many igneous rocks of unascertained composition.

The preservation in the St. Lawrence valley, and in the Great Lake district, of beds of loose laminated sands and clays lying below the till is due, as has been shown by Dr. Newberry, to the fact, that the ice was rising against the slope of the land. It had, in consequence, little erosive power, but advanced principally by the slipping of the higher portions of the ice over the lower. When it topped the southern water-shed of the valley of the St. Lawrence, its action produced a different set of phenomena, for its motion was down the slope of the land, and its erosive power was vastly increased. With this subject I shall not here deal, nor shall I attempt to trace the limits of the ice in its greatest extension, as that would lead me into a dissertation on the whole of the glacial period in North America, far beyond the scope of this paper.

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* American Journal of Science and Arts, vol. v., March, 1873.

As the ice melted back, it deposited the unstratified till under its receding foot, leaving a continuous mantle of it behind. Lying on the top of the till are seen scattered rounded boulders (E in section), often of great size, of granite, gneiss, and other crystalline rocks, that must have travelled from the Lawrentian hills in the far north. Amongst these, rocks of local origin are as scarce as in the till below those of distant derivation are rare. These foreign boulders are scattered over the surface, as if dropped by some agent that has left no other record of its movements. The rounded far-travelled blocks lie on soft unconsolidated beds that have not been disturbed. In some places, as on the top of a low hill on the Canadian side of the falls, I found great numbers of these blocks, and in some parts of northern New England and New York, great trains are found in lines along the sides of hills, as if stranded on a beach. They are found on the western prairies, according to Professor Hall, in long trains, "where, for many miles, the difference in elevation is not more than 50 feet; and here we observe long lines of boulders stretching away for miles beyond the reach of vision, as if once formerly a line of coast."* Speaking of the valley of the Hudson, Professor Hall says:—"In the vicinity of Albany and Troy, I have searched in vain for a boulder or pebble of granite, or of any rock older than the Potsdam sandstones in the deposits *below* the clay, while, in a period subsequent to the deposition of the clays and sands, boulders of granite are by no means rare."†

Only one satisfactory explanation has been given of the presence of these far-travelled blocks on the surface of the undisturbed loose beds of sand and clay, namely, that they have been dropped from floating ice, and most writers on the subject have concluded that they are proofs of the submergence of the land below the sea. There is certainly an area of land running from Lake Champlain northwards that has been elevated from below the level of the ocean since the glacial period, but there is no evidence whatever that the sea extended over the plateau of Lake Erie, and the entire absence of marine remains renders the supposition untenable. And if we follow the natural sequence of events that must have ensued during the retreat of the ice, we shall see that there is no occasion to call in the agency of the sea. For just as, during its advance, the ice from the north-east had blocked up the great valley of the St.

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* Natural History of New York, part iv., p. 321.

† *Ibid.*, p. 320.

Lawrence and changed it into an immense lake, so, during its retreat, it must have done the same. Probably it did so to a greater extent, not only because, in its retirement, it had left moraines and deposits of till, blocking up the deep channels draining into the Ohio and the Mississippi, but because, during the greatest accumulation of ice, the land northwards, and especially the area of the St. Lawrence, had been depressed, and an immense sheet of fresh water, dammed back by the lower part of the valley of the St. Lawrence, being still filled with ice, stretched south-westward and northward. On the northern shores of this great lake glaciers still came down from the Lawrentian hills, and gave birth to icebergs that floated southward, dropping boulders of granite, gneiss, and other crystalline rocks, on the bed of the lake, or stranding on its shores, and there depositing their freight. During this time were also formed many stratified beds of sand and gravel that lie above the till, and to it belong most of the deposits of the "terrace epoch" of Dana which were formed, not after, but during the glacial period.

Before leaving this branch of my subject, I must again advert, as I have done in previous papers, to the great importance of a proper appreciation of the effect of the stoppage of the drainage of the northern parts of the continents during the glacial period. It was not only a period of erosion and transportation of rocks, but of great fresh water deposits; and I fully believe that the fresh-water and inland sea beds that Professor Ramsay proves to have been deposited in old red sandstone and Permian times were due to former glacial periods, that of the Permian epoch being greater than the last one, and resulting in such a lowering of the level of the ocean, that there was great destruction of marine life by the increased salinity of the sea.

There are many proofs that the ice was thickest and highest during the glacial period in the bed of the Atlantic. That which advanced up the valley of the St. Lawrence came from the direction of Greenland, and the whole of the eastern coast of America, down as far south as New York, must have been blocked up by it. This is proved, not only by the many fresh-water beds and terraces due to the damming back of the rivers, but by the direction taken by the continental ice. Thus, over the higher summits of New England, the scratches point to the south-east and not to the east, as they would have done if the ice had been free to move directly towards the ocean. I think that this shows that the bed of the ocean was then occupied by ice, and it could not fail to be so, for to the land it occupied the position of a great valley, down which the ice from the north would naturally flow. I do not think, however, that the time of the greatest extent of ice in the sea-bed was the same as that of the greatest thickness of ice on the land; for, as the margin of the ice of the ocean-bed moved southward, it would cut off the moisture-bearing currents travelling towards the land, and gather to itself the precipitation from them. Thus, I think it was that the ice on the land shrunk back, at the time of the greatest extent of that which occupied the bed of the Atlantic; and we have, both in America and Europe, a period of land-ice, followed by one of fresh-water deposits and fresh-water borne icebergs.

I endeavoured to show in my paper, published in this journal in October last, that the ice from Greenland also reached the western coast of Europe. It passed across Iceland, and overflowed Caithness. Iceland is so hugely glaciated, that we may conclude the northern ice invaded it also; and, extreme as the view may seem, I can find no other satisfactory explanation of the fact, that the whole of the south of England is mantled by fresh-water glacial beds, than on the supposition that, at the height of the glacial period, the English Channel was blocked up to the south-west by ice that extended in an unbroken mass from Greenland. I sought in vain, before my last visit to North America, for a satisfactory solution of the presence of the fresh-water gravels and floated boulders of the south of England, and was driven to suppose that one or more barriers of land must have existed in the western part of the British Channel; but, after seeing how the ice in the bed of the Atlantic blocked up the water-shed of the eastern seaboard of North America, ten degrees of latitude further south, I have no difficulty in imagining that it may also have blocked up the English Channel, and caused the formation of the high and low-level gravels, the beds of the Rhine, and the floated boulders of Devonshire, Somersetshire, and Wales. I venture to predict, that evidence will yet be found of the encroachment of the edge of this ice from the north-west upon the Continent, probably upon the coast near Brest, and I also expect that traces will be discovered of the great flow of water that must have taken place, either round the south-eastern termination of the ice, or around the mountains of Britany, into the valley of the Loire.

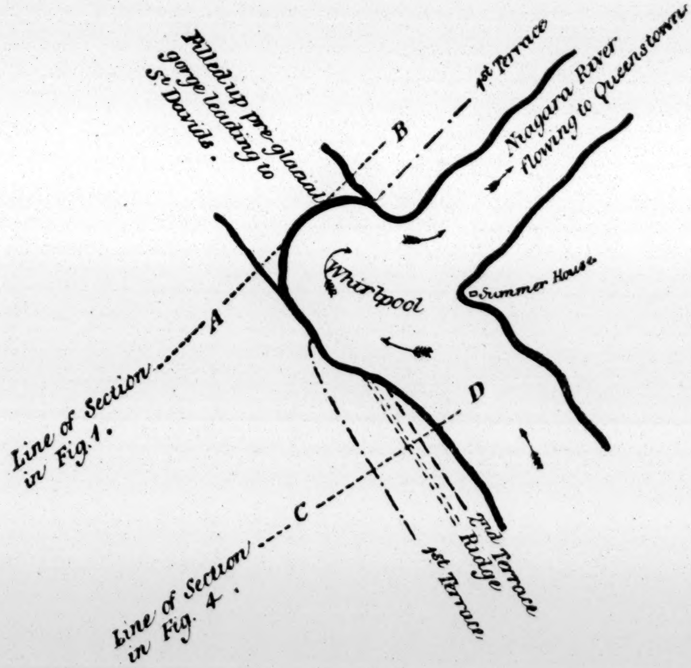
POST-GLACIAL PHENOMENA.

When the ice had retired so far back as to leave the channel to the St. Lawrence valley again open, the waters of Lake Ontario began to re-flow in that direction. From the whirlpool, northward, they did not run in their old channel, but took a more easterly course. This may have been because the lowest outlet through the moraines left by the ice was in that direction, as Dana has suggested; but I think it more likely that it was because the ice retired from the eastward first. If we look at a map of the southern side of Lake Ontario, we shall find that most of the rivers have been diverted in the lower part of their courses to the eastward, indicating that the cause was not one of accidental configuration of the ground, but some such general one as the early retirement of the icy barrier from the eastern part of the lake.

Wherever the river first commenced to flow, there it was likely to cut down through the rocks, for it would soon make for itself a channel through the loose drift lying on the surface; and between the banks of that channel it would be confined, and there only operate on the hard rocks below, just as, in copper engraving, the acid only acts on the plate in the lines cut through the soft wax covering it. To make clear the argument in the question we have to discuss, namely, how much of the gorge in which the river now runs has been excavated out of the solid rocks since the glacial period, I must, in the first place, direct attention to the sketch plan (Fig. 3) of the old and new gorges at the whirlpool, four miles below the falls. The sketch is founded principally on a small plan in Lyell's "Travels in North America," and partly from my own observations and sketches on the spot. I regret that I cannot give an accurate plan, and I could not learn that any complete survey has ever been made.

Standing at the summer-house, on the American side, at the point where the river takes a sudden bend to the eastward, I looked across the whirlpool to the old gorge opposite, and the question at once presented itself to my mind—from this point there are two channels downwards, one excavated before, the other after, the glacial period; to which does the one upwards to the falls belong? This question does not appear to have occurred to the authors of the theory, that the whole of the gorge through which the river now runs, from the falls to Queenstown, is post-glacial. But why might not the old pre-glacial river have excavated the gorge above the whirlpool, as well as the old one below it, and the present river have only cut back the gorge from Queenstown to the whirlpool, and, from that point upwards, have re-occupied and cleared out the old channel? On the face of it, the latter alternative seemed to me more likely, for the river above the whirlpool is running in a direct line for the old gorge, and is, moreover, about the same width as it is, the gorge to Queenstown being narrower. I found, with surprise, that this important point had been overlooked, and that it had been assumed, without discussion, that the gorge above the whirlpool belonged to the new, and not to the old river.

Fig. 3.

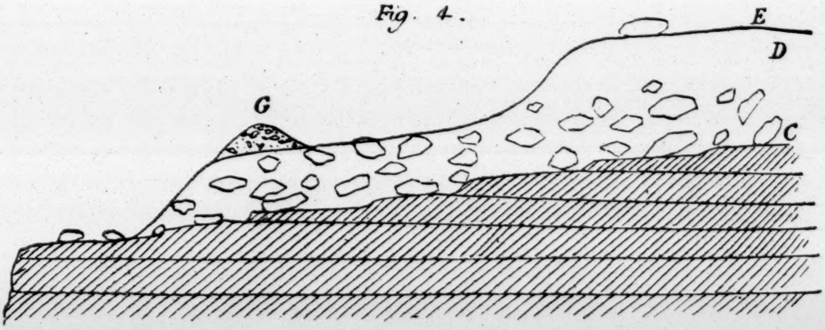

Filled up pre-glacial gorge leading to St Davids.
1st Terrace
B
Niagara River flowing to Queenstown
Whirlpool
Summer House
A
Line of Section in Fig. 1.
D
C
Line of Section in Fig. 4.
2nd Terrace
Ridge
1st Terrace

Plan of the old and new gorges of Niagara river at the whirlpool.

I determined at once to devote the time I could spend at Niagara to the elucidation of this question, and soon found some data bearing on the subject. Lyell and Hall both noticed the terraces formed in the superficial deposits, when the river commenced to cut back the new gorge from Queenstown. These mark its course when it flowed along the top of the plateau, and it seemed to me unlikely, that if the gorge above the whirlpool was pre-glacial, that the post-glacial river would have followed exactly the same line when it was bounded only by the superficial deposits that marked the older features of the country. I found that, above the whirlpool, the post-glacial river had run in different channels, having, apparently, often changed its course in the superficial deposits. Thus Lyell has described one of these deserted channels that ran from the muddy river to the whirlpool.* Another, I noticed, ran down from behind the town of Niagara-falls. In some places, the terraces and ridges that bounded the old river come down to, and are cut off by, the present gorge; at other places they retire back for at least 100 yards from it. They prove that, before the present river was confined in its rocky gorge, it often changed its channel, as rivers do now that run through superficial deposits over a nearly level plain.

On the Canadian side, a little above the whirlpool, two of these terraces come down to the gorge, and are cut off by it. Their direction is shown by dotted lines in the sketch plan of the river gorges at the whirlpool (fig. 3, p. 147), and the following figure is a section through them at the line C D, in plan.

Fig. 4.



Section through the line C D in Plan, Fig. 3, showing two terraces excavated in till and a river ridge capping the lower one. C, till with angular blocks; D, till without stones; E, rounded boulders of northern origin; G, river ridge.

There are here shown two river terraces, of which the highest and oldest has been formed by the washing off by the river of the clay without boulders (D), from that with boulder (C), leaving a level terrace, excepting where it is capped by the river ridge (G). The river at the whirlpool has cut back into the old gorge, clearing out the entrance to it and cutting off this terrace, but on the other side of the old gorge it re-appears and continues on, parallel to the course of the present river, and without any reference to the old blocked-up channel, across which it had evidently been at one time continuous.

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* Travels in North America, vol. i., p. 42.

This terrace must have been formed when the river was much wider than now, and flowing so slowly, that it had only power to cut through the unconsolidated sandy clay, without stones. The lower terrace marks a later stage, when, by the cutting back of the gorge, the river ran with a swifter current, at a lower level, and in a narrower channel, and cut through the lower till to the solid rock below. It marks the last stage in the present river's course, before it occupied the gorge cut through the Silurian limestones. The ridge capping the bank of the old river-bed must have been heaped up during floods. It is well exposed, though in danger of total obliteration, as it is being carted away for gravel. The stones in it are all rounded, like true river gravel. Mixed through it, often filling the interstices between the stones, are multitudes of fresh-water shells belonging to the genera *Melania*, *Limnea*, *Unio*, and *Cyclas*. I was able to confirm the observation of Lyell, that the assemblage of species is the same as that now found in the river above the falls. Amongst the stones of the present beach above Goat Island, I found shells of the same species as I did in the old ridge above the whirlpool, and in not much better state of preservation; indeed, excepting that I had labelled the boxes containing the different sets, I could scarcely now tell which were the older of the two. This assemblage of dead shells in the gravel of the beach differs, both in the older and newer deposits, from that found living in the present river, in that many delicate shelled species are scarce, or not found at all, owing, no doubt, to only the more robust shells being preserved. Thus, thin-shelled species of *Physa* abound in the river, but I did not find any of their shells, either amongst the stones of the present or the old beach.

A little above the whirlpool, the gorge widens out abruptly, as shown in plan, and the terraces and ridge are cut off by it, so that they cap the gorge, and are exhibited in section almost as clearly as I have depicted them in fig. 4. The river ridge, composed entirely of loose gravel and sand, is seen running to the edge of the cliff. The widening of the gorge extends for some distance beyond it, cutting off the upper terrace in the same manner. At this spot the upper layers of limestone project beyond the lower beds, just as they do now at the table rock at the falls, forming an overhanging precipice, so that the widening of the gorge cannot have been caused by weathering. I cannot conceive how the present river could excavate the gorge beyond the loose ridge of gravel, without washing both it and the terrace of unconsolidated till away; and it seems to me that we have here a proof, of what appeared before to be probable, that the river has at this point only re-opened the ancient gorge, the clay and stones that filled the widened part having been washed out by water from below, not from above, as would be necessary to excavate the gorge itself. I concluded, therefore, that the pre-glacial falls had been situated at least as high up as this point, and I thought that the narrowing of the gorge upwards, though it was still wider than that leading to Queenstown, might mark the commencement of the present river's work above the whirlpool.

On examining the gorge higher up, I however discovered that there were several places where it widened suddenly out, and at two of these I found similar proofs of the gorge not having been excavated by the present river. Thus, on the American side, between the railway and the suspension bridge, there are two or more widenings of the gorge, and I noticed a terrace of till at the upper one cut off by the setting back of the gorge. On the Canadian side, about a quarter of a mile below the suspension bridge, there is one of these sudden widenings or bulgings out of the gorge. Higher up a ridge of till, capped by river gravels containing fresh-water shells, marks a former channel of the river, and runs down about 50 yards from the gorge. This ridge does not wind round the widened part of the gorge, but runs down to it, and is abruptly cut off by it, similarly to the one above the whirlpool. Exactly the same argument may be used to prove that the present river has not cut out the gorge at this point, but only emptied it of the glacial clays and sands with which the old pre-glacial gorge was choked up. This example of a river ridge cut off by the re-excavation of an older gorge, is the nearest to the fall that I could find. From thence, upwards, the river terrace is back from the gorge, and uninterrupted by it.

The argument resolves itself into this form: above the whirlpool the gorge approaches both in direction and width nearer to the old one leading to St. David's than to the post-glacial one leading to Queenstown. That it is pre-glacial is strongly indicated by the fact of the post-glacial ridges being cut off by it in consequence of its re-excavation, whilst there has not been a single argument advanced in favour of the theory that it is post-glacial, which was simply founded on an assumption that does not bear investigation. The conclusion at which I arrive is, that the gorge was cut back from the whirlpool up to at least within three-quarters

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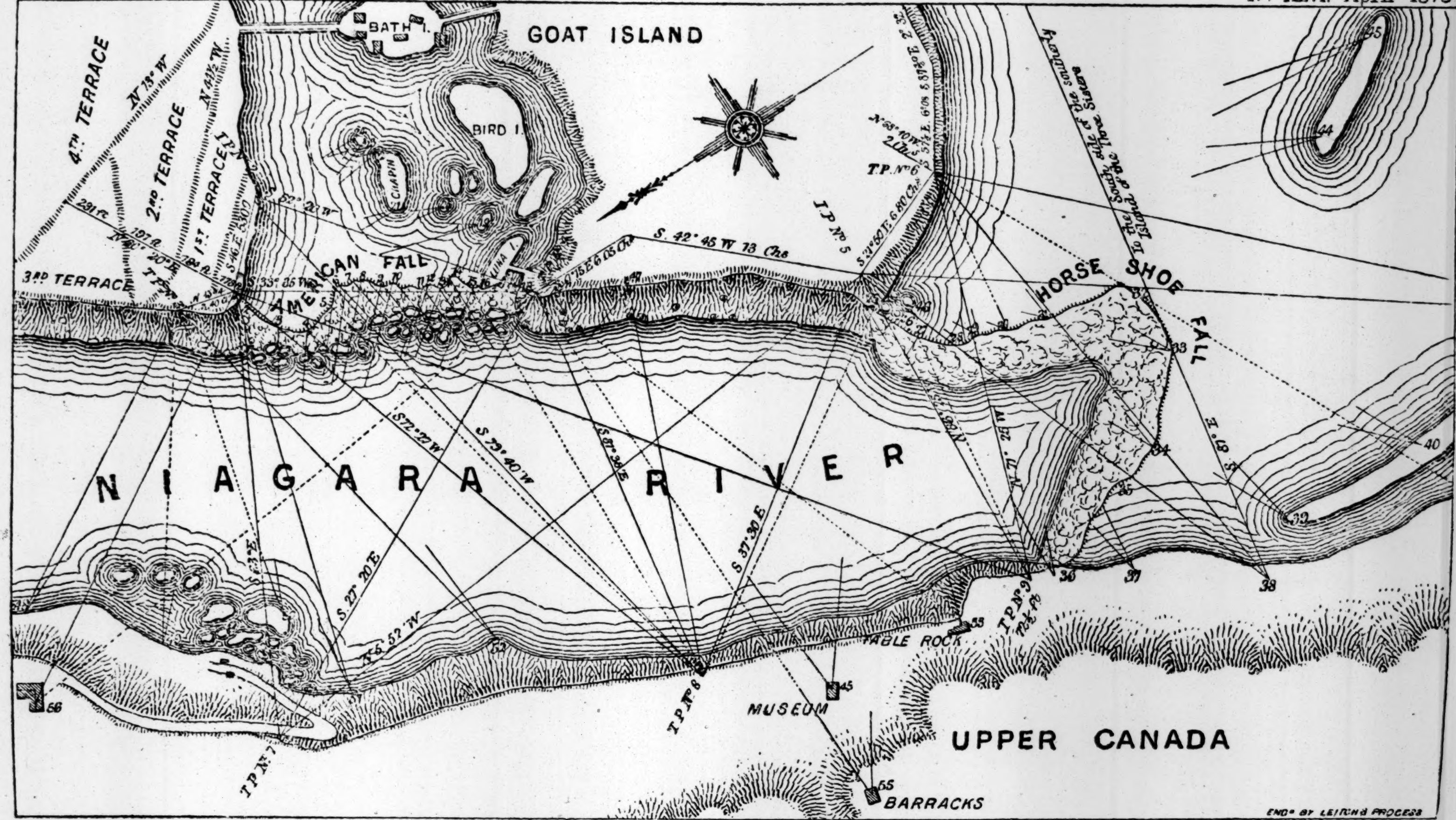

ENGᴰ BY LEITCH'S PROCESS

Trigonometrical Survey of the Falls of Niagara.
(Reduced from Plan published in Geological Report of New York, 1843.)

of a mile from the falls before the glacial period. It may have existed to within a few yards of the falls, for anything that can be seen to the contrary, whilst, in favour of such a supposition, there may be advanced the great width of the gorge up to the commencement of the horse-shoe fall, the very small indentation that the American fall has made in the side of the gorge over which it leaps, and the appearance the plan of the falls presents, that the river is now cutting back a much narrower gorge, one as narrow as that leading to Queenstown from the whirlpool.

I hoped to have been able to find at Goat Island some evidence bearing on this question, as both Lyell and Hall have described river gravels capping the till there, and also indications of a pre-glacial channel excavated in the silurian rocks, but the whole of the island was covered with a glassy surface of ice produced by the frozen spray from the cataract, that made it most difficult to get about the sloping banks, and masked the beds I wished to examine. On the Canadian side there rises a high ridge of till, overlain by a thick bed of boulder clay, with large stones; and on the American side, there is what appears to be a continuation of this ridge, now cut through by the river. Around this ridge, on the American side, there are indications, as I have already mentioned, that the river once flowed. Goat Island seems to be a remnant of this ridge, and I imagine that it has been pierced, not from above, but from below, that it overlaid the pre-glacial gorge, and was undermined in the same way as the clay filling up the end of the old gorge has been at the whirlpool. This and other questions that arise I must leave to observers with more time at their disposal, and a more favourable season of the year to make their investigations than I enjoyed. The observations that I made are the result of three days' holiday from business, and I am sure, from what I saw, that three months close application would not exhaust the many points of interest that present themselves. The geologists of Canada and New York could not have a more interesting question to work at than this, and if they did no more than correctly and fully map out the gorge, the terraces, and the river ridges, they would confer a great benefit on geological science. The absence of such maps I found to be a great drawback to my investigations. The only part of the gorge that has been surveyed with minute accuracy is that at the falls. This was done by Professor Hall, assisted by competent engineers, in 1842, and permanent marks were at the same time fixed in the rocks. The first step of a new survey should be a trigonometrical re-measurement of the rocks at the fall. This, compared with that made thirty-three years ago—one-third of a century—could not fail to afford data for calculating the present rate of retrocession, and would be a fitting compliment to the veteran geologist under whose auspices the first survey was made, and to whom the whole scientific world is a debtor for a lifetime spent in geological research.

Whenever that survey be made, I believe it will decide that the present river is cutting back the gorge much more slowly than Lyell estimated; that, instead of one foot yearly, the retrocession is not more than, if it is as much, as one foot in ten years, and that, allowing for the comparative softness of the rocks below the whirlpool, we shall have to put back the occurrence of the glacial period to at least 200,000 years ago, if we conclude that the whole of the gorge, from the falls to Queenstown, has been excavated since that time. But if the conclusion at which I have arrived is correct, that the gorge, from the whirlpool to the falls is pre-glacial, and that the present river has only cut through the softer beds between Queenstown and the whirlpool, and above the latter point merely cleared out the pre-glacial gorge in the harder rocks,—20,000 years, or even less, is amply sufficient for the work done, and the occurrence of the glacial epoch, as so measured, will be brought within the shorter period that, from other considerations, I have argued has elapsed since it was at its height.

Simply looked at from a geological point of view, the time occupied may not seem important, and it has been usual for geologists to ask for an unlimited duration, though, even from that standpoint, it is difficult to reconcile the small amount of denudation that glacial moraines exhibit with the remote antiquity that some physicists assign to them. In Ohio and Illinois, the mounds of the old Indians do not look more recent than the ridges and gravel hills of glacial origin, and in some parts cannot be distinguished from them until excavations are made into them. In England I know we have a school of geologists who have taught that the river valleys of the south of England have been excavated since the glacial period; but wherever we find undoubted glacial deposits, as in the north of England and in Scotland, we find them scarcely altered from the time when they were laid down.

But the student of the succession of changes in the organic world will have a serious difficulty removed, if it be proved that the glacial period occurred not more than twenty thousand years ago. In the northern temperate zone, so far as we can learn, there has been little variation in the animal or vegetable world since the glacial period. In the tropics, the formation of specific differences has been probably more rapid, but in northern Europe, the species now living differ but little, if at all, from their pre-glacial ancestors. Some of the large mammalia have become extinct, but the fauna and flora are essentially the same as they were before the glacial periods—that is, though some species have died out, we have no proofs of any new ones having come in. There is not a single example of a distinct species having been formed since that time, though some varietal differences may be detected. Even man himself has, I believe, varied but little, physically, since pre-glacial times.

In the paper already referred to, published in No. 44 of this journal (Oct., 1873), I assumed that the arguments brought forward by distinguished geologists, to prove that the palæolithic implements and the mammalian remains found with them were post-glacial, were founded on a sound data. There were great difficulties to be surmounted if that conclusion was correct; but the published sections of the superficial beds, at Bedford and at Hoxne, seemed to admit of no other explanation. Since then I have been able to examine for myself some of the supposed post-glacial beds, and to devote more time to the study of the whole of the valley gravels in the south of England, at the bottom of which the palæolithic implements and mammalian remains are found. The conclusion at which I have arrived is, that so far as the British Isles is concerned, palæolithic man, the mammoth, the woolly rhinoceros, and the hippopotamus are entirely pre-glacial, and that the great and distinct break between the palæolithic and neolithic deposits in that area was caused by the culmination of the glacial period, when to the north of a line drawn irregularly from Lynn Regis, in Norfolk, through Birmingham westward, nearly the whole country was covered by land-ice, that destroyed the mammalian bones and the palæolithic implements, excepting where preserved in fissures and caverns, or in a few spots in the eastern counties, to which the ice did not reach; and when, to the south of that line, a great lake or sea of fresh water, dammed back by the ice that blocked up the German ocean to the north, and the British Channel to the west, covered the pre-glacial remains beneath a mantle of beach gravels as it rose and fell.

Dr. Falconer long ago argued that the older cave mammalia were pre-glacial; Mr. Tiddiman has found a human bone beneath glacial *débris* in Yorkshire; in America Professor Whitney has announced the discovery of a pre-glacial human skull, and I hope soon to be able to lay before geologists the evidence I have collected, that I think proves that the tools of palæolithic man in the British Isles are all of pre-glacial age. Nearly all ethnologists are agreed that the representatives of palæolithic man are the Eskimos of the far north, and probably, in glacial times, they held much the same relation as they do now to more civilised communities, living further south in more congenial climes, and I have suggested that the records of a glacial civilisation still exist in the statues and cyclopian ruins of some Pacific Islands.*

If we have to go back 200,000 years to the glacial period, the small amount of change in the organic world, and the slow progress of civilisation northwards, from its southern home, are difficulties not easily surmounted by the evolutionist, for he has not unlimited time at his disposal. This world and its inhabitants do show signs of a beginning, and he will have to put that beginning back far beyond the time that physicists and astronomers will allow him, if 200,000 years scarcely takes us one step backward in the long succession of changes in the organic world, of which we have proofs in the strata of the earth's surface. These difficulties will be greatly lessened if the period of the glacial epoch has to be put back only 20,000 years; and, so far as the excavation of the gorge of Niagara affords a scale of measurement, there is no reason to ask for a longer time.

II. HEREDITY.†

It is recorded that towards the end of the seventeenth century two Highland chieftains were condoling with each other on the unpleasant fact that "the law" had penetrated within fifty miles of their mountain-fastnesses, and that soon not a spot would be left where the claymore was the only arbitrator between man and man. In our day, minds of a certain class perceive with alarm that order after order of phenomena is being brought within the grasp of what the scientific world not very happily terms "law," and that the very last strongholds of the arbitrary—the regions where force was supposed to be created, and where events arose without any natural sequence—are now invaded.

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* Naturalist in Nicaragua, p. 269.

† Heredity: a Psychological Study of its Phenomena, Laws, Causes, and Consequences. From the French of TH. RIBOT. London: H. S. King and Co.

Heredity and Hybridism: a Suggestion. By EDWARD W. COX, S.L. London: Longmans and Co.

The doctrine of heredity, as applied to man, is one of the latest conquests of the scientific spirit, and, as such, it is still viewed with dislike and suspicion.

That like produces like has, indeed, been an article of popular faith for untold centuries, and has been embodied in the proverbial philosophy of all but the merest savages. If a son resembles his father in person, in habits, or in character, we are told that he is a "chip of the old block," or reminded that "the apple does not fall very far from the tree." But, with curious inconsistency, man often revolts at conclusions even when he has loudly proclaimed their premises. So long as the law of heredity was tacitly assumed to apply merely or mainly to racers and greyhounds, to short-horns and to crève-cœur pullets, it was admitted as a matter of course, and facts which placed the induction upon a yet firmer basis were welcomed. But now the result appears: now it is plain that the law embraces not brutes only, but man also, and that it extends from his outward structure to his intellectual powers, his vices, and his virtues; there is dire confusion, and a wish to retract former admissions were it possible. Such men as M. Ribot and Mr. Galton, who have collated facts bearing upon this important subject, and who, consistently following up the clue, have pointed out the only legitimate interpretation, have incurred no small share of obloquy.

There have been, hitherto, two hypotheses professing to account for the varying aptitudes, faculties, passions, and tastes of men. The one—to which we may refer hereafter—put forward by the revolutionary philosophers of the eighteenth century as a corollary of their doctrine of universal equality, held that all men were essentially and originally alike, and that the diversities which they exhibit were due solely to training, education, early associations, and other post-natal circumstances. The experience of every man is quite sufficient to render any formal refutation of this view needless. The other hypothesis, though still widely entertained by divines and moralists, and accepted as a point of conventional orthodoxy, is, if possible, still more outrageous. It regards every man, at least,* as the creator of his own character. Genius, heroism, crime, insanity, every exuberance, every deficiency, and every aberration of our inner life, are self-caused,—coming into existence without any regular and unvarying antecedents. Let us take as a specimen the following passage, which M. Ribot quotes from Heinroth :—

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* We are not aware that this doctrine has been extended to the lower animals, who have their differences of character as well as man.

" Insanity is the loss of moral freedom ; it never depends on a physical cause ; it is not a disease of the body, but a disease of the mind, a sin. It neither is nor can be hereditary, because the thinking *ego*—the soul—is not hereditary. What is transmissible by way of generation is temperament and constitution, and against these he must react whose parents were insane if he would not himself become lunatic. The man who, during his whole life, has before his eyes and in his heart the image of God, need never fear that he will ever lose his wits."

It is scarcely necessary to say that if we admit this hypothesis we must, to be consequent, reject not this or the other of the teachings of Science, but abandon altogether the scientific spirit and the scientific method. We may not be able to foresee—as we predict a solar eclipse or a transit of Venus—whether or no a newly-born child will prove a genius. The laws involved are beyond our present grasp; possibly altogether beyond the powers of the human intellect. But if we maintain that the result is independent of law we may as well deny the conservation of force, question the axiom that like antecedents will be followed by like consequents, and abandon our faith in the uniformity of Nature. For us, then, Science has no longer any existence. But the advocates of the hypothesis assert that man makes himself a genius, a lunatic, an idiot, a criminal, or a common-place citizen, in virtue of his own " free will,"—an explanation which is far from mending their case. Not being either a Calvinist or a fallen angel* we shall not enter into such regions. To pursue a metaphysician into that Serbonian bog is as bootless as to chase a Cateran into his own wilderness. Still we may remark that this free-will view leaves the question unaffected. The difficulty, like a sturdy beggar in some parts of Germany, is merely " abgeschoben,"—laid on a barrow and wheeled into the next parish. Why should the " will " of John produce effects which the will of Thomas does not, except there be in the two some difference, innate or connate ? And whence springs this difference ? Is it, too, self-caused ? How, moreover, can the will create faculties which are in their origin and development synchronous with itself?

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* Milton happily represents his devils debating on free will.

But perhaps the Accidentalist—for so we must term him who denies the law of Heredity—may ask, in turn, whether that law is not also open to the charge of merely pushing away to a little distance a difficulty which it cannot solve? You derive your genius from your father, who, in turn, received it from your grandfather. Is not this like the Hindoo Astronomy which supported the earth on an elephant, and the elephant on a tortoise, but left the tortoise to struggle as he might for a footing among the Immensities? Certainly not, for here we must point out as a happy coincidence that the doctrine of heredity was not formally promulgated until after the great law of organic evolution had been made known. Obscurantists have, indeed, sneered at modern science as denying—by the mouth of Messrs. Galton and Ribot—what she had just declared by the mouth of Darwin, Wallace, Haeckel, and Schmidt. The joke is too poor to be unpardonable. The doctrines of evolution and of heredity are not contradictory, but harmonious. When we account for genius by pronouncing it hereditary, we do not mean to say that it suddenly made its appearance in some remote ancestor, or that it has been from eternity an heirloom in certain families. We conceive of it as being formed, in the course of generations, by small successive increments. A man of fair average intellect and good constitution marries a woman similarly endowed. He leads a life free at once from excesses and from asceticism, from torpor and from exhaustive over-work,—free, above all things, from anxiety. Suppose one of his children, born under such favourable circumstances, well nurtured and well educated, repeats the process. The grandchildren will, in all probability, show marks of decided superiority. If, now, such conditions are continued, the result in the fourth or fifth generation may be a Shakspeare, a Bacon, or a Humboldt. If we reflect how rare must be the concurrence of conditions needful for this gradual elevation of a family, we need not wonder at the scarcity of genius.

The question may be raised, on the other hand, not why children inherit the faculties and dispositions of their parents, but why such resemblance is not more complete and more uniform? What, simply, is the cause of that striking diversity often recognised in one and the same family? In short, admitting the general principle of heredity, what are its conditions, its modes of action, and its limitations?

Here it must be admitted that, although M. Ribot has collected a most valuable mass of facts, and has given us many interesting reflections upon their bearings, he still leaves much work for his successors. The case is one of extreme complication. Every human being, and every animal of the higher orders, has two parents whose influence is blended in a manner which to us appears, so far, inexplicable. We do not even know whether one parent, as a rule, determines the outward form, and the other the constitution and character; or whether, in each of these points, a resemblance may be detected to both. It has been suggested that something may here be traced resembling the chemical law of combination in multiple proportions, the male or the female parent, according to its general or momentary predominance, affecting more or less the character of the offspring. Thus children of the same parents might differ from each other, just like compounds formed from the same elements, but in varying proportions, as $A + B$, $A + 2B$, $2A + B$, &c.

Here, then, already is scope for great diversity. But in addition to the influences of the immediate parents, sometimes acting in conjunction and sometimes in opposition, and varying with their degree of vigour and mental state, both at the moment of procreation and during gestation, we have the phenomena of atavism. It has long been remarked by physiologists, physicians, breeders of cattle, &c., that attributes which have been observed in grand-parents, or in more remote ancestors, but which have lain dormant for one generation or for more, suddenly reappear. In this fact M. Ribot sees a certain analogy with alternate and serial generation, as it may be observed in the salpæ and the medusa.

We have, further, what is called indirect heredity, "the representation of collaterals in the physical and moral character of the progeny." Between uncle and nephew, or grand-uncle and grand-nephew, and even in still remoter degrees, we not unfrequently find "striking resemblances of conformation, face, inclinations, passions, character, deformity, and disease." Such resemblances have been ascribed to chance, and used as an argument against the very existence of heredity. M. Ribot very justly points out that "indirect heredity is only a form of atavism—a form which is rarer and less easy of apprehension than direct atavism, but differing from it only in appearance."

The most perplexing case is that which M. Ribot designates as "heredity of influence," a form rare indeed, but still thoroughly established. Any female animal may produce offspring resembling not her present, but a former mate, who may have been dead for years. However interesting this phenomenon may be as a physiological problem, it is too rare among the human species to have any important bearing upon our subject.*

But when the action—often conflicting—of all these forms of heredity has been duly taken into account, there is still a residuum, to which M. Ribot gives the name of "spontaneity." Occasionally, though not often, a man appears, eminent for good or for evil, whose genealogy throws no light at all upon his peculiar attributes, or seems even in flat contradiction to what the law of heredity would at first sight lead us to expect. But we must remember, that in many instances nothing is known of the ancestry or of the collateral relatives of a man of genius. Before he rose to celebrity these points excited no curiosity; when his reputation is established, the inquiry comes too late. It is very true, as M. Ribot remarks, that "merit does not belong exclusively to history." If we look upon the intellect, not as a single force, but as a complex of faculties, we shall find little to perplex us in the phenomenon of spontaneity. Suppose a family who have possessed some of the attributes of greatness, but who, in virtue of a principle equally true in psychology and in mechanics, that "nothing is stronger than its weakest part," has remained in obscurity. Let a man of this family marry a woman whose faculties are the complement of his own. It is possible that a child of such a couple may combine the defects or the weaknesses of both parents, and we have then the case of spontaneous imbecility, or criminality. But it is also possible that he may combine the excellences of both, and burst upon the world as a spontaneous genius. As to the laws which determine whether of these two cases shall be realised, we are still utterly in the dark. Perhaps the state of the parents at the moment of procreation is the clue to the difficulty.

Again we must remember, that even if we consider the intellect as "one and indivisible," it is far from being the only faculty needful for the attainment of excellence, even in the fields of pure science. Combined with it there must be the moral faculties* of patience, perseverance, and concentration. The will must be strong enough to overcome all distracting temptations, whether in themselves good or evil. Lastly, there must be constitutional energy and endurance. Failing these, the man will merely leave among his friends the conviction that he might have achieved greatness, if ——. We once knew a physician, resident in a small country town, who, from time to time, startled his associates by some profound and suggestive idea—some brilliant *appercu*. But a constitutional languor prevented him from ever completing an investigation, or from leaving the world one written line.

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* There is a form of heredity still more obscure, and requiring further investigation. If a female animal, pregnant by one male, has during pregnancy frequent connection with another male, the offspring may, to some extent, resemble the latter.

Nor need we feel greatly surprised if the son of an illustrious man proves, intellectually or morally, a failure. The world often expects from such a one too much. He is lost to sight amidst his father's fame, like Mercury in the blaze of the Sun. Further, we must remember that men of the very highest order often fail to leave any posterity at all,—a circumstance too common to be merely casual, and pointing, we think, to the truth, that in producing a genius a family exhausts itself. Is it, then, very wonderful if men—eminent, though not of the foremost rank—should leave progeny inferior to themselves?

The suggestion of Mr. Cox—that every organism is produced by the union of two germs, one supplied by the male and the other by the female parent—seems to us certainly to throw a clearer light upon some of the phenomena connected with hybridism. "All living forms," he writes, "are *double*; that is to say, they are not one whole constructed as a whole, but two halves joined together, always more or less unlike,† having points of resemblance and of difference obviously derived some from one parent and some from the other, and which indicate a greater share by both parents in the product than has yet been assigned them. The suggestion of a double germ (one supplied by each parent) accounts naturally for hereditary characteristics and tendencies, mental and bodily, and for the mysteries of hybridism. . . . If it be true that Nature's method is by the union of two germs, practical horticulture, no less than physiology and psychology, will have secured a firm foundation, from which a new departure may be made into the region of speculation, no less than into the domain of fact." The learned author asks whether, in such cases as the "two-headed nightingale," there is "duplicity of structure in any portion of that part of the united structure which is common to both. Are the bones formed by the junction of two bones? Are the muscles duplex? Are there two sets of nerves? Is there any abnormal condition of the muscles or tendons indicating the original presence of two formative forces acting together?" He suggests the careful examination of the next double-headed calf or lamb produced.

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* Are we right in drawing such a rigid line of demarcation between the intellectual and the moral powers? Do they not rather exist in what might be called "mixed solution?"

† Entomologists occasionally meet with moths one-half of whose body, taken laterally, is male, whilst the other half is female. We have ourselves captured a specimen of this kind.

We cannot, however, agree with Mr. Cox in his views on hybridism. The arguments he applies to the mule—which is usually, though not invariably, barren—would apply also to the *léporide*, and to certain hybrid birds which are fertile. Nor can we see that this hypothesis would at all aid us in understanding the "heredity of influence."

M. Ribot, though in part dealing with questions already handled by Mr. Galton, takes wider ground. He treats not alone of the heredity of genius, but of instincts, of the sensorial qualities, the memory, the sentiments and passions, of national character, and of morbid psychological heredity. To examine in detail all his views is a task which we must leave to the reader. To some of his remarks upon instinct we feel compelled to demur. Thus, he declares that "A bird hatched in a cage will, when given its freedom,* build for itself a nest like that of its parents, out of the same materials, and of the same shape." Has this experiment ever been tried with a pair of birds hatched in confinement, and then building where they cannot associate with others of their own species? The alleged stationariness of instincts is another questionable point. For how many thousand—or even hundred—years have we possessed detailed and trustworthy descriptions of the nests of birds and insects? Suppose a naturalist were to perceive, in the architecture of a wasp or of an ant, something entirely new,—an improvement on the part of the insect,—would he recognise it as such, or would he not rather pronounce it something old which his predecessors had failed to observe? Mr. Wallace has very justly pointed out that if the nests of birds are unvarying, so are the tents of nomadic Arabs, the wigwams of Red-skins, and the huts of Greenlanders. The author maintains that "we may have the same organisation with different instincts," and remarks, in confirmation,—"Birds have their beaks and feet as their only instruments for building, yet how great are the differences of the form, architecture, and position of their nests." This instance is not happily chosen. The beaks and feet of birds differ so much in structure that they alone will go far to account for the varied architecture of their nests. Of what use would be the feet of a duck, or the beak of a parrot, in constructing the pendent nest of an oriole?

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* "When *given* its freedom." We regret to see this slovenly expression finding its way into a work like that before us.

In treating of the heredity of the sensorial faculties, the author refers to the prevailing short-sightedness of the Germans, and ascribes it to their studious habits. We believe that the German type is mainly to blame, and that a nation equally studious, but making use of the Latin character, would escape.

The section treating on the heredity of the intellect might, we think, have been advantageously extended. Several cases, which decidedly support the view taken by the author and Mr. Galton, are omitted. The scientific world knows two Liebigs, two Fresenius', two Mitscherlichs, two Mulders, two Thenards, two Persoz', two Becquerels, two Berthollets, two Sainte-Claire Devilles. It is strange, also, that no mention is made of such men as Laplace, Lagrange, Latreille, Lavoisier, Lacepede, Gay-Lussac, Guyton Morveau, Wœhler, Lamarck, Blainville, Oken, Carus, Faraday, Dalton, Boerhaave, Swammerdam, Harvey, Volta, Nobili, Biot, Scheele, Schleiden, Chevreul, Agassiz, Dumas, &c. Surely the lineage of many of these men could be accurately traced, and could scarcely fail to throw an additional and welcome light on the subject. For, admitting the law of heredity in its broader outlines, we have yet to ask whether "genius" be general or special? Would the man who achieves greatness in one department have been equally great in another, and is the choice of studies merely determined by circumstances? Could an eminent chemist, if placed in youth under other influences, have become an eminent lawyer, divine, classical scholar, musician, or man of business? M. Ribot and Mr. Galton appear to answer this question in the affirmative. Hence, if a man has become illustrious in any respect, they cite—as cases confirmatory of the doctrine of heredity—any of his relatives who have in anything risen above the average level. Thus, in connection with the great German poet Heinrich Heine, Mr. Galton mentions his uncle, Salomo Heine, an eminent banker and philanthropist. Along with Francis Bacon, M. Ribot notes his half-brother Nathaniel, "a clever painter." To the name of Jeremy Bentham, legist and moralist, the author appends—"His *brother*, General Samuel Bentham, a distinguished officer. His *nephew*, George, an eminent botanist, president of the Linnean Society."

Of course this view renders it very easy to find evidence of hereditary genius. Our own observations lead us, however, to the antagonistic opinion—the speciality of genius. To us it appears that men of universal powers—"admirable Crichtons," who adorn whatsoever they touch—are of all psychological phenomena the rarest. The man who in one faculty rises high above the average of his fellows is very likely, in accordance with the great law of compensation, to fall below them in others. The mind, or the brain, if the latter term be preferred, has its idiosyncrasies no less than the stomach. To take a striking instance:—Towards the beginning of the present century, at a German educational establishment of the old type, where pupils were gauged solely according to their proficiency in classics, there was a boy who was the acknowledged dunce of the school. Sneered at by his companions, and denounced by the masters as little better than an idiot, his declaration that he intended to become a chemist was received with a general outburst of contemptuous laughter. But the boy knew his own speciality. His success in chemistry was yet more decided than his failure in classics, and when he passed away from our midst he left the name of Justus von Liebig, second to none in the annals of Science. There are, of course, groups of sciences or of studies within which the choice of an aspirant may be determined by what is commonly called accident. Thus a great chemist, doubtless, if placed under slightly different influences, might have reached equal distinction as a physicist. The same powers which render a man eminent in botany would have done him equal service in zoology, palæontology, or physiology. But to maintain that a distinguished cultivator of any of the above sciences could therefore, *if he liked*, have attained corresponding success as a barrister, a preacher, or a parliamentary orator, seems to us hazardous in the extreme. In these latter spheres eminence absolutely requires great power of expression, and the faculty which is aptly—though clumsily—called ready-wittedness. Now a man of science may be very poor in expression and very slow in thought, provided only that he thinks profoundly and originally. If some evil star had placed Henry Cavendish in the bar, the pulpit, or, we may add, at the merchant's desk, would he not have made a most deplorable figure?

The author handles the heredity of the imagination and that of the intellect in two distinct sections. Under the former he gives the genealogies of poets, painters, and musicians; and under the latter men of science and prose authors, even novelists. This arrangement seems to us objectionable. The gifts requisite to produce a prose work of fiction are surely more akin to the faculties of the poet than to those of the mathematician, even although the imagination plays no unimportant part in scientific discoveries. It is interesting that, in treating of painters and musicians, M. Ribot recognises—implicitly at least—that speciality of talent which he overlooks among philosophers. With scarcely an exception, when tracing the lineage of artists and musicians, he enumerates those only of their kindred who were themselves artists or musicians.

Into the interesting and suggestive chapters on the relation between the physical and the moral,—on heredity in connection with the law of evolution, and on its psychological, moral, and social consequences,—we must, though with reluctance, abstain from entering. To two, however, of the practical applications of the truths brought forward by Mr. Galton and M. Ribot, we will briefly call attention.

The doctrine of Heredity throws a needful light upon the question of national education. What shall we gain by catching every little boy or girl and consigning him or her to school, whether "Board" or denominational? Some of us, especially the neophytes of the movement, expect too much. It was held by Helvetius, in accordance with the revolutionary hypothesis of universal equality, that the differences, intellectual or moral, between man and man were simply due to early training. It was believed that genius could be raised to order, and the market supplied with original thinkers as easily as with early peas or hothouse grapes. Nor has this delusion totally disappeared. M. Ribot, as a matter of course, sees that the power of the schoolmaster over the raw material put into his hands is strictly limited. The mind of the child has been likened to a blank sheet of paper, but it is paper upon which certain characters may be traced with readiness, others with difficulty, and others, again, not at all. Or if we adopt the Socratic view, that the educator is like a sculptor who cuts out a beautiful statue from the crude block, we may say that every such block has a determined cleavage, which limits him, in each case, to the production of a certain class of forms. M. Ribot maintains that upon minds of the highest and lowest classes the influence of circumstances in general, and of education in particular, is comparatively small:—"We restrict education, as we think, within its just limits, when we say that its power is never absolute, and that it exerts no efficacious action except upon mediocre natures. Suppose the various human intelligences to be so graduated as to form a great linear series, rising from idiocy, the bottom of the scale, to genius, which is at the top. The influence of education is at its minimum at the two ends of the series. On the idiot it has hardly any effect: unheard-of exertions and prodigies of patience and ingenuity often produce only insignificant and transient results. But as we rise towards the middle degrees this influence grows greater. It attains its maximum in average minds, which, being neither good nor bad, are much what chance makes them; but as we ascend to the higher forms of intelligence we see it again decrease, and as we come nearer to the highest order of genius it tends towards its minimum." So long, however, as persons of "mediocre natures," intellectual and moral, constitute, as they are always likely to do, the great majority, the importance of education—both in a national and in an individual point of view—remains unshaken. Men who have not that magnificent spontaneity which enables its possessors to open new epochs in the history of Science, may yet, if properly trained, be capable of useful research. It will never be theirs to discover a law of gravitation, an atomic theory, a conservation of forces, or a doctrine of organic evolution; but they may discover and analyse new compounds, natural or artificial; they may detect new forms of organic life, observe their structure and habits, and determine their geographical distribution.

If, then, education can do such things,—if it can call into being discoverers of the second order, inventors, and appliers,—it must always constitute a main element of national greatness, and stands in no need of fictitious or exaggerated recommendations.

Not less important is the bearing of the law of heredity upon crime and its treatment. There are evidently two very distinct classes of criminals,—the casual offender, who in some unguarded moment has yielded to temptation, and the normal hereditary villain, such as the Liverpool "corner-men." As regards the latter, we have to consider not merely the garotter or ruffian brought before the courts, but his descendants, actual or possible, of whom the bulk will as surely be criminals as the children of a consumptive person will be prone to pulmonary disease. Attention has lately been drawn to the fact that the ancient sanctuaries of

England, which were nests where the vilest criminals swarmed and multiplied, are to this day characterised by the low moral type of their inhabitants. Whitefriars, Fulwood's Rents, and certain districts of Westminster, are sufficiently well known in London. Scarcely an episcopal city but has a similar rookery beneath the very shadow of its cathedral. A young woman of remarkably depraved character, who infested the districts on the Upper Hudson at the beginning of the present century, has left eighty direct descendants, of whom one-fourth are convicted criminals, whilst the rest are drunkards, lunatics, paupers, prostitutes, or otherwise useless members of the community. If the lineage of criminals is inquired into, as it doubtless henceforth will, we shall find that crime literally reproduces crime. So long as we imagined that the criminal, if placed under more favourable circumstances, might have grown up a virtuous man, we were justified in a policy of leniency, and might entertain the hope of reclaiming the offender. This hope—except as regards the casual, incidental criminal—is now taken from us. The hereditary ruffian must be "stamped out," by death or by penal servitude for life, as the circumstances of the case may indicate.

We do not set ourselves to work to train tigers and wolves into peaceful domestic animals; we seek to extirpate them. Why should we act otherwise with beings who, if human in form, are worse than wild beasts, since they slay and maim out of pure wantonness? Education, moral and religious influences, may doubtless arrest the formation of criminal races, and bring them back to a normal condition; but can we allow a criminal family to go on for four or five generations, murdering intelligent, moral, industrious citizens, whilst we are trying experiments in reclamation? To educate the son of a garotter or of a "corner-man" into an average Englishman is about as promising a task as to train one of the latter into a Newton or a Milton.

III. THE LATE TRANSIT OF VENUS.

By RICHARD A. PROCTOR, Author of "Past and Coming Transits," &c.

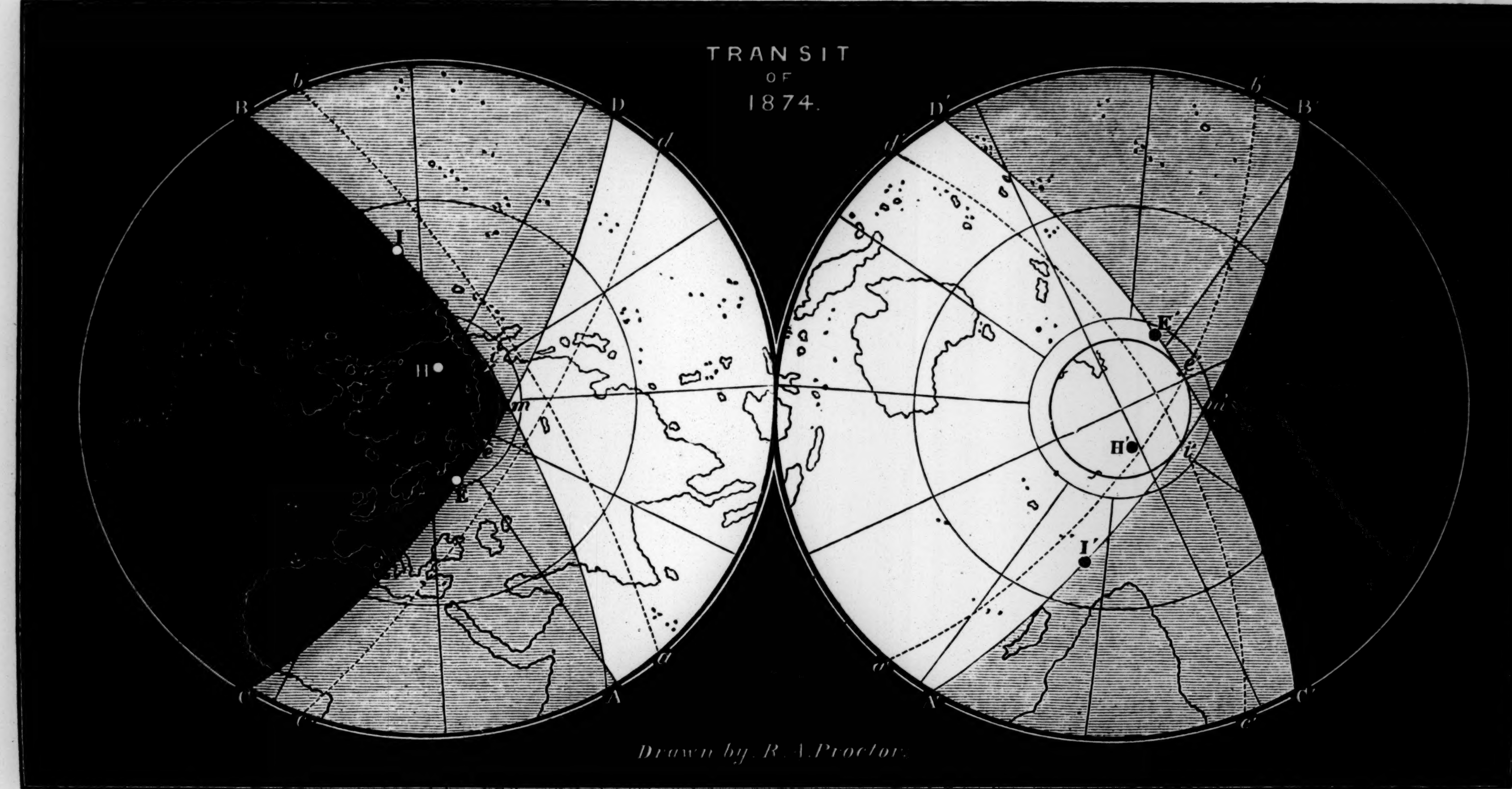
AT the moment of writing, news has been received from all the stations selected for observing the late transit, excepting two French stations—St. Paul's Island and Campbell Island—and though as yet the news from most of the stations is far too imperfect for any attempt to be made to calculate the solar parallax, while from others we only have news that observations were successfully made or otherwise, yet we know enough already to be able to form an opinion as to the general success of the operations. The result cannot but be considered as on the whole extremely favourable. It was certain, or all but certain beforehand, that there would be fine weather at some of the stations and bad weather at others; and the feeling was universally entertained amongst those best competent to judge that, if fine weather prevailed at half the selected stations, astronomers would have every reason to be satisfied, provided always that the fortunate stations were not all in one or two regions, or in the same hemisphere. It was also considered that, if one half of the various methods on which reliance was placed should succeed, the result would be quite satisfactory. It will appear, from the facts I am about to bring before the reader, that these hopes have been much more than fulfilled. A large proportion of the stations had weather either altogether favourable, or sufficiently so to enable the observer to secure satisfactory results; while all the methods proposed for observation have been successfully applied (unless the Janssen turning-wheel arrangement be regarded as a distinct method rather than a modification of Delisle's).

For convenience, it will be well to consider the several methods separately, rather than to take the stations in order—in fact, no otherwise can clear or satisfactory ideas be obtained of the success of the whole plan of operations.

We may consider the methods in use as divided into three broad classes—the Delislean, the Halleyan, and the mid-transit method. This classification includes the photographic and heliometric operations, as well as the spectroscopic method for observing external contact. For, both photography and heliometry may be regarded as means for determining the position of Venus at given instants, either near the beginning or end of transit, or near the middle of her chord of transit; and observations of external contacts, either at the beginning or end of transit, come under the head of Delislean operations, while the observation of the interval of time between the two external contacts is a Halleyan operation. But also the Halleyan and mid-transit methods can be considered together, because all good Halleyan stations are good stations for observing mid-transit. Let us take the classes in the order indicated, viz., first, the Delislean; secondly, the Halleyan.

Speaking generally, it may be said that the English scheme was the only one which made direct provision for Delisle's method. Originally, as most persons know, the idea was that no other method was available at all; and, accordingly, we find not only that our official folk invited England to provide exclusively for Delisle's method, but assigned to other scientific nations their Delislean parts. Russia was to fortify the region for observing earliest ingress (around the point I in the plate), and France also was to provide stations for this phase, England occupying the Sandwich Isles (aided perhaps by an American party there, and another at Tahiti, unless it so chanced that America reserved her strength for the transit of 1882, her share in which was also assigned her by our official astronomers). France was to strengthen the forces near I′, the region where retarded ingress was to be observed, though here again England undertook the chief part. For the region around E′, where egress occurred earliest, England made herself entirely responsible; and, lastly, she undertook to occupy Alexandria, leaving to Russia the provision of stations on Russian territory around E, the place where egress occurred latest.

If this programme had been carried out, England would have reaped the chief honours, always supposing that the plan of operations had been justified by the actual conditions of the proposed work. England would of herself have made sufficient provision for applying Delisle's method, both at ingress and at egress, other nations taking only a subordinate part in the arrangements for either phase. Of course, if it had been discovered after the event that the proposed plan of operations took no account of at least one-half the chances of success, and left regions unoccupied which were even more important than those considered in the English official programme, England would have reaped something far different from honour. Other nations might then have been able to say that trusting in the unquestionable skill and



TRANSIT OF 1874.

Drawn by R. A. Proctor.

A B and A′ B′, separate sunlit and darkened hemispheres at ingress; along *a b*, *a′ b′*, sun 10° high at ingress.
C D and C′ D′, separate sunlit and darkened hemispheres at egress; along *c d*, *c′ d′*, sun 10° high at egress.
I, I′, are the Delislean poles for ingress; E, E′, those for egress; H, H′, the Halleyan poles.

the presumable diligence of official astronomers in England, they had been misled, and had missed a fine opportunity of advancing scientific knowledge. But as the matter presented itself six years ago, there seemed every reason to believe that England was about to effect an achievement more than worthy of her ancient fame in such matters.

It turned out, however, rather inconveniently, that "some one had blundered." Other methods were available than Delisle's—were even better suited to the occasion. Other regions than those indicated were of extreme strategic importance, one of them being indeed the key of the whole position. So far was England from having provided adequately for the whole scheme of operations—that is, from having prepared to do more than her fair share of work—that actually certain regions in British territory, which it behoved her and her only to occupy, had not even been mentioned in the official programme.

When this was discovered, it fortunately was discovered at the same time that other nations were willing to take at least as large a share in the work as England. It was also found that the other nations (whether convinced by the reasoning urged in England against the official arrangements, or for whatever cause) attached so little value to Delisle's method proper, that they might not unsafely be expected to devote almost their whole strength to Halleyan and mid-transit operations. This was convenient in more respects than one. It not only ensured adequate provision for the method hitherto so strangely neglected; but it left it in the power of English official astronomers to adhere to their original programme (or nearly so) without absolutely endangering the success of the whole scheme of operations. As they were apprised early of the views of other nations, official astronomers quickly adopted this course. They even overacted their persistent adherence to the Delislean as the sole available method. Where the durations of transit could be observed, they said that instead of observing the duration they would observe the absolute time of the beginning and ending, which is a very different thing—at least so they asserted. Nay, so completely were the other nations about to provide for Halleyan and mid-transit observations, that our official folk felt free to add new Delislean stations, or rather stations professedly Delislean; for some among them were open to the objection of being excellent also as Halleyan stations. As regards the neglected region in British territory (North India), official astronomers would at first make no concession; then they would have a photographic station there; next they would make the station also a Delislean one; and, lastly, they conceded all that had been asked, a thing hitherto unheard of in the annals of officialism.

While, however, no inconsiderable proportion even of the English preparations were directed to Halleyan operations (more or less disguised), it was to England, almost alone, that Delislean operations were left. The Americans declined *totidem verbis* to occupy any station where the whole transit could not be seen; the French refused to occupy the Mauritius or Suez (calling forth from English official astronomy an expression of strong dissatisfaction), and the Germans, though they occupied a station in Persia where the ingress could not be seen, yet specially provided for photographic work there during the middle of the transit. The Russians alone, having provided eleven Halleyan stations in Siberia, consented also to have observers at Delislean stations nearer to Russia in Europe, extending around E′ to the Black Sea, Caspian Sea, and Sea of Aral.

The only Delislean stations properly so termed near I were those occupied by the three English parties under Captain Tupman, in the Sandwich Isles, at Honolulu, Atooi, or Kanaü, and Owhyhee (or Hawaii). At the two first observations were successfully made, but at Hawaii, the least important fortunately of the three, the observers Forbes and Barnacle had bad weather. Detailed information has not yet reached us respecting the observations made by Tupman and his party at Honolulu, but it is known that the Janssen apparatus failed. The reader is doubtless aware that this apparatus was so arranged as to allow sixty photographs to be taken at intervals of a second—a large circular plate being so turned that picture after picture would be taken around the border of the plate; so that if the turning began half a minute or so before the true moment of internal contact, one or other of the pictures would depict Venus in true internal contact. Whether the manager of this organ-grinding arrangement (to adopt a humorous simile of the Astronomer Royal's) began turning too soon or too late is not known, for the telegraphic announcement from Honolulu states simply "Janssen failed." Sixty photographs were obtained there, presumably by this method, and these either represent Venus drawing nearer and nearer to the position of internal contact, or else they were all taken after she had broken away from the sun's edge. Unfortunately the telegram indicates only too clearly the probable cause of failure, in these words, "internal contact uncertain several seconds."

This points to a disturbed condition of the air, which would render all the observations unsatisfactory. One hundred and twenty micrometric measurements were made at this station; but if the cusps were affected by a disturbed atmosphere, it is to be feared that little reliance can be placed on them.

If the observations made in the Sandwich Isles should prove to be unsatisfactory, the only observations of ingress to be depended upon are those made at stations outside the region B *m* D, in Japan, North China, and the adjacent part of Siberia. At Port Possiet and Wladiwostock, near the western shores of the Sea of Japan, the ingress was well seen by Russian and American observers; but the acceleration there only amounted to about 6½ minutes, whereas at the Sandwich Isles it amounted to 11 minutes. From Yokohama and Nagasaki, the news which has hitherto arrived has been somewhat confused. First, news arrived that the Americans had been partially successful at Nagasaki, but had been troubled by clouds; next we heard from Nagasaki that the French, under Janssen, who had intended to observe at Yokohama, had been successful, presumably at Nagasaki. Then a telegram arrived correcting the Nagasaki date for Yokohama; and a writer in the "Times" seems to have regarded this as signifying that the French had observed at Nagasaki instead of Yokohama; but, possibly, what was really meant was that the French observed at Yokohama, though the telegram came from Nagasaki. Next news came from Yokohama through an Austrian source, that good observations had been made there, which the "Times" interpreted as meaning that an Austrian party had been stationed there, though until then nothing had been heard of such a party. Lastly, Janssen telegraphed that good observations had been made at Nagasaki and Kode. It seems probable, as I have remarked in the "Observer," that in reality one series of good observations was effected at Yokohama by the French under Janssen, and another at Kode; that the news was forwarded through Nagasaki, and that the Austrian news relates to the success at Yokohama, while Janssen's later telegram about observations at Nagasaki may relate to the work of a subordinate party. My reason for so interpreting the telegrams is that the Russians announced complete success at Yokohama, while the Americans announced only a partial success at Nagasaki; now the French party under Janssen achieved full success, which implies that they were at the station favoured with the better weather.

In any case it appears that accelerated ingress is fairly provided for; though at the best English stations for that phase Janssen's method failed, and the ingress was not very satisfactorily seen.

In the opposite region around I′ there were three English observing parties, two in Kerguelen Island, and one on Rodriguez; while Lord Lindsay's station at the Mauritius, though not specially intended for Delislean operations, was even better placed than the Rodriguez station. I shall speak more at length of Lord Lindsay's operations further on. Here it is only necessary to say that his party missed ingress, though otherwise successful. At Rodriguez ingress was successfully observed, but as yet no details have reached England. It is known that the various parties stationed on Kerguelen Island were successful; but the nature of their success is not known, all the news yet received from that dismal island having been derived from the interchange of signals between the parties stationed there, and a passing ship.

It would appear that a fair success has attended the employment of Delisle's method, as applied to ingress. At least, whatever defects may appear hereafter in the results by this method will be due to the inherent defects of the method itself, requiring as it does the observation of contact when the sun is not far from the horizon. Janssen's contrivance, by which it was hoped that this difficulty would be removed, failed entirely for ingress.

Turning next to egress, we have first to consider the operations around the point E′, where accelerated egress was to be observed. The stations provided by England for observing this phase were in New Zealand, though the Government Observatory at Melbourne, and the Observatory at Sydney were fairly placed. It must be remembered, however, that everything depends on securing observations at both ends of either Delislean base-line by similar methods and by observers similarly trained. The only stations thus provided for were those in New Zealand; and unfortunately bad weather prevented the observation of egress at any of these stations. The American party, stationed at Queenstown, in Otago, were able to observe and photograph all the transit except egress, but the English parties were not even favoured with this partial success—or rather, I should say, with a degree of success which, in their case, would have been but partial; for to the Americans the observation of egress was a matter of small importance. It is impossible not to sympathise with Major Palmer, on account of this unfortunate end to a campaign for which he had made very complete arrangements. As a member of his party, writing from Wellington, remarks, "it certainly seemed not too much to expect that, towards evening of a summer day in December, one of the finest months in the year (in New Zealand), an hour's clear sunshine might be vouchsafed to at least a considerable part of a colony somewhat larger than Great Britain and famed for the beauty of its climate. Unhappily, these hopes and expectations were crushed by a day of pitiless weather. All through the 8th, and till mid-day on the 9th, from every quarter of both islands, telegrams conveying the same dismal tidings of mist and rain, cloud, gloom, and falling barometers, poured in on Major Palmer, the English chief, warning him that unless some sudden and unlooked for change should very soon take place, the careful plans to which he and his colleagues had for so long given their time and energies would prove to have been made in vain. No change came, and failure was the result." "To crown their trouble, the day after the transit was provokingly, almost mockingly, fine. Some excellent sun-photographs, which were taken at Burnham on that day, showed how carefully the dry plates had been prepared, and how successful this, the least certain branch of the work, would have been. That the choice of stations was judicious, and that all was done that could be done with the means at command, is the opinion expressed everywhere." This opinion will certainly be shared in England also; nor will astronomers be slow to accord to Major Palmer their fullest sympathy for his misfortune. As was remarked of Le Gentil's failure in 1761, Major Palmer has "experienced one of those mishaps which assume to the man of science all the proportions of a real misfortune—to have traversed so large a portion of the globe, to have endured all the weariness, all the privations, all the perils of a long sea-voyage, and to have been able to effect nothing."

The Germans at the Auckland Islands, a station superior in value to any in New Zealand, achieved great success. Ingress, indeed, was obscured, "but ten minutes later the sun shone out, and, the sky remaining clear till the end of the transit, some 150 photographs, as well as the observation of egress, were obtained." The French at Caledonia Island saw everything except the egress. No news has yet arrived from Campbell Island, where the French had a station. If success was achieved there also, then egress has been well provided for; though the failure in New Zealand remains scarcely less to be deplored, because of the special arrangements made there for securing observations directly comparable with those made by the Government parties in Egypt.

Turning next to the region around E, where retarded egress was to be observed, we have to consider results of a mixed character. At all the best stations, viz., those occupied by the Russians, bad weather prevailed. The Russian Delislean observing parties were spread over Western Siberia, and included such important stations as Erivan, Tiflis, Taschkent, Astrakhan, Ornsk, Blagowestchensk, &c. At some of these stations the retardation would have been fully twelve minutes, whereas, at the English stations in Egypt and North India, the retardation amounted only to ten minutes. At Ispahan, where a German party was stationed, there would have been a retardation of 10¼ minutes; and as the results would have been directly comparable with those obtained at the Auckland Isles, a very valuable Delislean success would have been obtained had good weather prevailed at Ispahan. Unfortunately, though some good mid-transit photographs were secured, the contact at egress was missed through clouds. A Russian party, but not provided with first-class instruments, were successful at Teheran. But the chief success, so far as retarded egress was concerned, was obtained in Egypt and in North India, a circumstance which makes the ill-success of Palmer in New Zealand the more unfortunate, as it was with his observations that the Egyptian results were to have been compared. In Egypt thick haze prevailed on the important morning, until within a few minutes of the moment of contact. Then, however, the sun passed clear of the low-lying haze, and the contact was well observed. It was at first announced that Captain Abney had succeeded in getting a photograph of the contact by the Janssen instrument, but this news turned out to be incorrect. He just missed the actual contact; though it would appear that the moment of contact can be approximately determined from the photographs.* The German observers stationed in Egypt also made very satisfactory observations. At Roorkee, in North India, egress was well observed (as also the whole transit).

On the whole, the Delislean observations for egress have been fairly provided for. The special English plans have been defeated by bad weather in New Zealand; but the observations at Melbourne and Sydney will combine tolerably well with those made in Egypt and at Roorkee. The German observations in the Auckland Isles will combine admirably with the German observations in Egypt.

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* It seems tolerably clear that in future applications of the Janssen arrangement provision must be made for a longer interval than one minute.

Before passing to the operations by other methods, it may be well to consider the general Delislean results, and the lessons they teach us as to future operations. It is, in the first place, certain, that if only Delislean operations had been provided for, as in the original programme, the measure of success achieved would have been very far from satisfactory. Early ingress does not appear to have been well observed, and Janssen's method failed, so that the complete ingress operations are to some degree imperfect. The English special operations for observing egress have completely broken down through the failure in New Zealand. The success of the Germans is a strong point, but one success counts for little in a process where everything depends on reducing the final error through the *number* of successful observations. It is to be noted that the German success results entirely from the fact that the Auckland Isles were occupied. These are among the island groups to which I directed attention in 1869. Chatham Island, from which we hope to hear of good American egress observations, was another—rejected by the Astronomer Royal, because, by a miscalculation, he set the solar elevation 7° too low. Campbell Island, where the French have a station (not yet reported from), was yet another of these islands. All of these were specially named by me as suitable for applying the Delislean method at egress, and also for observing the whole transit. Their occupation by America, France, and Germany, and the success of the Germans (even though we should not learn that the Americans and French had also been successful), sufficiently justify my suggestions, and more than meet the comments made on these suggestions by official persons. On the other hand, the partial failure of the English Government operations in no sense supports my position, except in so far as to justify the anxiety I expressed. Bad weather might equally have thwarted the arrangements I proposed, which other nations carried out.

Turning now to Halleyan and mid-transit operations, we have a series of excellent results to consider.

In the first place, let us examine the northern successes. At Nertschinsk, where the duration fell short of the mean by fully 15½ minutes, the Russians observed the whole transit with excellent telescopes, and obtained a number of measures and photographs. This success is particularly gratifying, as Nertschinsk was the best of all stations for applying Halley's method. So far back as 1869 I called special attention to this point. But even so late as February, 1873, the Astronomer Royal declined to believe that observations would be made there. "Nertschinsk," he said, "is a station in high latitude, nearly 1000 miles from the nearest sea. I presume that its climate is truly continental. At St. Petersburg, in the winter, the sun sometimes is not seen for several weeks together. I suppose that the same may happen at Nertschinsk. I doubt greatly the probability that any observations can be made there." This he assigned as the sole reason why England should not occupy Southern Halleyan stations. Though four years had passed since I pointed to Nertschinsk as a suitable northern station, the fact remained still unknown to the person principally responsible for the English arrangements that *eighty per cent* of winter days are clear at Nertschinsk. Of course Russia occupied this excellent station. She also occupied ten others in the region extending thence to the Sea of Japan, obtaining more or less complete success at six of them. The Americans were successful at Wladiwostock and Possiet, the Germans at Chefoo, the French in Japan and at Pekin. The duration was not, indeed, secured at all the northern Halleyan stations, though it was at most of them; but where either ingress or egress was missed, the position of the chord of transit was effectually secured by mid-transit photographs and heliometric measurements. At Roorkee, in the long-neglected Indian region, the whole transit was observed and photographed under Col. Tennant's skilful supervision. The Germans photographed mid-transit at Ispahan, the Russians at Teheran. The whole transit was also observed by amateur astronomers at Kurrachee, Indore, and Calcutta, a fact rather showing what ought to have been done by official astronomers in England to strengthen the north Indian position, than (in all probability) adding much to the value of northern Halleyan operations.

In the southern hemisphere corresponding successes have been already reported, though as yet we have not heard from some of the best southern stations, nor have we sufficient news of the nature of the success known to have been achieved at Kerguelen Island. But the Germans were successful in securing mid-transit photographs in the Auckland Isles, and the Americans at Otago and in Tasmania. At Sydney, Melbourne, and Adelaide, the chord of transit has been well secured. At Melbourne, in particular, the observations may be regarded as presumably most valuable, on account of the Government observatory there. I do not know whether Mr. Ellery was able to make use of the instruments (belonging to the Astronomical Society), which Mr. Lockyer had so generously presented to him after the Indian eclipse of 1871. If so the Astronomical Society, though it should reclaim its property, may be disposed to pardon the slight mistake made in this matter. From the two French stations in St. Paul's Island and Campbell Island (two of my myths I suppose*) no news has yet been received. At Rodriguez the whole transit was seen, and mid-transit well photographed. The French also secured more than two hundred daguerreotypes at Caledonia Island, and these will prove exceedingly useful.

But the most complete mid-transit success was that achieved by Lord Lindsay's party at the Mauritius, where photographic arrangements much more satisfactory than those made at the Government stations were combined with heliometric observations of mid-transit and spectroscopic observations of exterior contact, neither of which methods had been provided for at all by our official astronomers.*

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* I have been in considerable doubt as to the localisation of my "geographical myths." Admiral Richards was sufficiently ready to describe me as suggesting "geographical myths," but he has never indicated what he really intended by that remark. Of southern islands which I recommended, if accessible (and which I supposed, after Admiral Richards's rebuke, to be inaccessible, to say the least), the Germans have occupied one, the Americans another, and the French two others; while the French say, in their report, that they would have occupied more stations in the southern seas, if they could have supposed England would have left the field open to them, as she actually did. Another, Crozet Island, was to have been occupied by America, having been pronounced accessible by experienced sealing merchants, but bad weather prevailed for some time after the *Swatara* had reached the islands, and having to convey all the American observing parties, she was compelled to proceed on her way without effecting a landing there. (England, with ample time, and colonial possessions close by, might very easily have occupied this station.) Macdonald Island proved indeed to be barely accessible, and was not occupied, though this was not proving it to be a geographical myth. I do not know whether Royal Company Island and McQuarie Island are myths; but they are certainly marked in admiralty charts. Possession Island was described by Admiral Richards himself as easily accessible. Sabrina Land was the selected station for the transit of 1882. Where, then, are the "geographical myths?" Great indignation was expressed with me when I said that the islands in south seas seemed to be described by the Admiralty as myths or realities, "as might be most convenient to those in authority;" and it was absurdly stated, that in so saying I was accusing the Admiralty of making false entries in their charts. The exact reverse is the case. I believe in the Admiralty charts; but the statement that my proposed stations are geographical myths I consider inaccurate. It is not possible to land on myths, or to set up observatories on myths. As I explained to the Astronomical Society, I attributed nothing dishonourable to Admiral Richards; his myths were a mistake—that is all. I should have thought his remark ought long since to have been withdrawn (as publicly as it was advanced), and not without some sort of apology. But officialism sometimes overrides good manners.

On the morning of the transit, at Mauritius, the sun rose concealed by clouds. Minute after minute passed without any sign of a break in the cloud-bank behind which the sun lay hidden. The important period of ingress, from first external contact to first internal contact, passed without a single opportunity of observing the motions of Venus as she entered on the sun's face. Had Lord Lindsay's party been relying upon the views entertained by astronomers five years ago, according to which only the Delislean method could be applied, their whole scheme of operations would have already failed, for the egress at Mauritius was not worth observing for that method. But the failure thus far signified only that one method out of three which the party had hoped to apply had failed them. We may say, indeed, that Halley's method had also failed, seeing that the duration of the transit could not now be determined; but the essential object in Halley's method is to determine the position of the chord of transit, and it still remained possible that this end might be secured. Fortunately the clouds which had so long concealed the sun cleared partly away about one hour after transit began, and, though the sun was visible only for a few minutes, photographs and measures were obtained. It was not till 8 o'clock in the morning that it became fairly fine, and from that time the course of observation steadily proceeded until the end of the transit.

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* It seems to me that Lord Lindsay's recent work in the cause of science is worthy of more than ordinary recognition. It is no new thing, indeed, for men of wealth and leisure to devote large sums to the advancement of science, though even in this respect what Lord Lindsay has done is noteworthy, seeing that the expedition fitted out by him will involve, first and last, a cost little short of the entire amount devoted to the British Government expeditions. The remarkable feature in the present case is the personal activity displayed in the cause of science by one who might well have been content with the contribution of some fifteen thousand pounds to a single scientific expedition. Moreover, it is to be remembered that this is not by any means the first of Lord Lindsay's services to science. On the occasion of the eclipse of December, 1870, Lord Lindsay fitted out an expedition at his own expense, all the preparations being much more complete than those made for either of our Government expeditions. On that occasion he accompanied his party, and took an important share in the work of observation. The next great astronomical event in which he assisted was the total eclipse of December, 1871. On that occasion he fitted out an expedition at great expense, the results obtained by which were the most important ever obtained in eclipse observation, seeing that Lord Lindsay's party stationed at Baikul secured a series of photographs of the corona which finally established the solar nature of that wonderful object. Lord Lindsay did not himself join the expedition to India, but he took a large part in the work of analysing the results then obtained. A laboratory was fitted up by him in London, where, in company with Mr. Davis (to whose skill the success of the operations was in large part due) and other skilful assistants, he investigated carefully all the details of the photographs obtained in India. Even at that time preparations were being made and experiments carried on for the expedition to observe the transit at the Mauritius. Lieut. Gill was placed as superintendent over Lord Lindsay's observatory at Dunecht, where all the instruments and methods to be employed during the transit were carefully tested. At the laboratory in London experiments were made by Lord Lindsay and Mr. Ranyard on the peculiarities of solar photographs, in order that the best method of photographing the progress of the transit might be adopted. Lord Lindsay not only took a share in such work, and in superintending the preparations at Dunecht, but visited continental astronomers, physicists, and instrument-makers, inquiring into the qualities of various methods, processes, and instruments, in order that every available means for securing the best results might be employed. Lastly, he was not content to send out the expedition thus carefully provided for, but himself went to the Mauritius and shared in the work of observation.

Lord Lindsay's place was in the photographic room. "I took," he says, "271 plates, out of which number perhaps 100 will be of value: cloud and the high temperature of the camera were much against me, the temperature varying from 96° to 100°. With the heliometer Mr. Gill obtained five complete determinations of greatest and least distance of the centres of the sun and Venus, besides nine measures of cusps, and two determinations of the diameter of Venus near the end of the transit. Dr. Copeland obtained fifteen measures of least distance of Venus from the sun's limb, and ten measures of cusps. The last internal contact was observed successfully, as also the last external contact.

I come next to a point which I would willingly pass over, but that the history of the transit would be incomplete without some account of it. It had been shown by me, in 1869, that Cape Town would be an excellent station for observing the middle of the transit. A letter published in the "Times," for February 15th, from Mr. Dunkin, informed astronomers that the egress of Venus was satisfactorily observed at Cape Town by Mr. Stone, Astronomer Royal at the Cape. The satisfactory observation of egress at Cape Town unfortunately counts for very little more than—say—the satisfactory observation of a sun-spot on the morning of the transit. The valuable stations for egress were those already considered, where egress was either notably accelerated near E′ or notably retarded near E. Cape Town is remote from either region, and observations of egress there had scarcely any value whatever. But Cape Town as a southern mid-transit station was absolutely better than any station occupied either by our own country or by others. Mid-transit photographs secured there, especially with all the advantages of a well-provided national observatory, would have been invaluable: so, also, would have been heliometric measures of Venus at the time of mid-transit, and afterwards until the end of the transit. It appears only too certain, from the absence of all mention of any such work in Mr. Dunkin's letter, that absolutely no provision whatever had been made for these most important purposes.* If the opportunity of utilising the Cape Town Observatory for mid-transit observations had simply been overlooked, little need have been said. That would have been no novelty, unfortunately. But special attention had been directed to the value of the station. In reply to a letter by Admiral Sir H. Cooper Key, inquiring whether Sir G. Airy had made arrangements for photographing mid-transit, the answer came that the method had been amply provided for; yet at the very best station for the purpose no provision had been made, though the station was exceptionally suitable, because of the Government Observatory there and the presence of skilled astronomers. This would have been unfortunate as a mere case of negligence, but in its real aspect the matter is much more serious. It will not readily be forgotten.

In summing up the results of Halleyan and mid-transit operations, we must distinguish between contact observations, photographic results, and heliometric measurements. We must also draw a distinction between the various modes of photographing the transit employed at different stations.

It seems probable that in future transits less reliance will be placed on contact observations than on photographic work. It is true that the results obtained during the recent transit show that the phenomenon of the "black drop," which in 1761 and 1769 occasioned so much trouble, depends on instrumental imperfections and atmospheric disturbances,† and can be practically eliminated by employing good telescopes and choosing stations where the sun will not be too low at the time of either internal contact. Nevertheless, a "personal equation" comes in, depending on the fact that the eye itself is part of the optical arrangement for observing contact, and thus the observed moment of contact depends—to the extent of three or four seconds at least—on the observer's idiosyncrasies. This equation may be estimated by practice on models; but it must always remain doubtful how far, in the excitement of transit observation, the personal equation remains the same as in the calm of "model transit" operations.

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* It is impossible not to connect this with what happened in the case of the important total solar eclipse of April, 1874, when totality lasted more than four minutes. On that occasion, though the track of total shadow passed close by our Cape Colony, Mr. Stone received no assistance whatever towards the proper observation of the eclipse. He had not even an equatorial telescope, but was obliged to observe with an altazimuth telescope "borrowed from Mr. H. Solomon," to which Mr. Stone attached a spectroscope with "wrappers of wash-leather," for want of more suitable appliances.

† Mr. Stone's ideas on this subject, on which so much stress was laid in 1868, have been entirely overthrown by the recent transit observations. Of all whose results have reached us, Mr. Stone himself was the only observer of skill who, with a good telescope, saw any approach to the "black drop" required by his theory. Certainly he saw and pictured what accorded most perfectly with his own ideas; but that only shows how likely preconceived opinions are to make the observer fancy he sees what he thinks he ought to see.

Heliometric measurements, again, seem inferior to the instantaneous work of photography. On any reasonable assumption as to the skill of the observer, it is impossible to believe that he can measure the position of Venus with an accuracy comparable to that with which the photographic picture can be measured, if only that picture is clearly defined, and not affected by imperfections such as would render the process of measurement uncertain. For instance, specks and stains on the photograph do no harm. A contraction of the film in photographs on glass would be mischievous, as also would be the effects of so-called photographic irradiation, if measurement depended on the size of the photographic image of the sun as affording a scale of measurement, while of course any optical defects would be fatal. But if such sources of error as these last can be in any way avoided, then photography must take its rank as *facile princeps* among the available methods for dealing with transits of Venus.

Now here, again, we approach one of those delicate questions unfortunately so numerous in the history of recent transit operations. English official astronomers were content to take the opinion of Mr. De la Rue, a skilful photographer of celestial objects, doubtless, but in the first place likely to view this matter solely in its photographic aspect, and secondly (as is manifest by his own account of the qualities of the method he advocated), unaware of the exact astronomical requirements for success. In this method, that used in the Kew Observatory, the image formed at the focus of the object-glass is optically enlarged before it is received on the photographic plate, and consequently its proportions depend on the instrumental adjustment, so that the only means of determining the scale of the sun picture is by a reference to the picture itself. But if there is any photographic irradiation or shrinkage of the collodion film, the sun image will be affected; in other words, *the scale of measurement will be falsified*. Now it is asserted by nearly every one who is at once competent to pronounce on photographic matters and acquainted with the mathematical aspect of the question, that there is no such accuracy in the outlines of the sun and Venus, obtained by De la Rue's method, as is requisite in the delicate problem of measurement involved. The photographic delineation ought to be not only the best possible, but ought to be practically perfect. What, however, do we hear (when it is too late) from the very person who was responsible for the employment of the best available method, and at whose instance not only the English, but the Russian and German, arrangements for photographing the transit had been selected? The Astronomical Society is calmly informed that the French method of photographing the transit (on silver) is *infinitely* more exact than the method of photographing on glass. I do not know in what sense the Astronomer Royal used the word infinitely; but he is a mathematician, and therefore probably used the word in its mathematical sense; that is to say, regarding the trustworthiness of the French photographs on silver as represented by some finite quantity t, the trustworthiness of the photographs taken by the English, Russian, and German Government parties, is in the Astronomer Royal's opinion,—

$$= \frac{t}{\infty} = 0.$$

We cannot blame Mr. De la Rue for proposing a method thus rejected as utterly untrustworthy. He was not the responsible person. Dr. Rutherford, in America, advocated the same method, which probably seems to the photographer the best available. But the American astronomers responsible for the photographic arrangements rejected this method, showing by their calculations that it could not be trusted. Our official astronomers have also found this out. But there is this trifling difference—American official astronomers made the discovery before selecting a method for photographing the transit; ours made the discovery a month after the transit had taken place.

The Americans met the difficulty, not as the French did, by taking daguerreotypes, but by using an object-glass of great focal length (40 feet). It is manifest that with such an arrangement, the true focal length being known, the scale of the sun picture is determined independently of the apparent size of the solar image. For the focal image is as large as a disc which, seen at the focal distance, would look just as large as the sun; and the sun's apparent diameter at the time of transit being known, as also the true focal length, we can calculate how large such a disc would be. The photographic picture may be slightly larger or slightly smaller through photographic defects,—expansion by irradiation or shrinkage all round the border; but, as we have no occasion to measure the diameter of the disc, this is a matter of perfect indifference. All we require to know is the distance, in any picture, between the centres of the discs of the sun and Venus, and whether the outlines of these discs have contracted or expanded the positions of their centres can be accurately determined. The great difficulty to be surmounted consisted in the construction of suitable mirrors for the heliostats. An ordinary telescope, 40 feet in length, was out of the question, since no available machinery could direct such a telescope continually towards the sun. The construction of a flat mirror was a matter of extreme difficulty. As Prof. Newcomb pointed out—"The slightest deviation from perfect flatness would be fatal: for instance, if a straight-edge laid upon the glass should touch at the edges, but be the 100,000th of an inch above it at the centre, the reflector would be useless." But with such opticians as Alvan Clark and his sons this difficulty was not found insuperable, and the mirrors provided for the eight American parties were to all intents and purposes perfect.

Lord Lindsay selected the same method. The mirror for his heliostat was constructed by Repsold, and worked excellently.

We must, then, in considering the photographic results, attach chief—if not sole—value to the successes obtained by the Americans and Lord Lindsay using the long focal method, and by the French using Daguerre's process. Fortunately a sufficient number of results have been obtained by both methods, and in both hemispheres, to ensure the determination of the solar parallax to a much greater degree of accuracy than heretofore. I think it is not too much to hope that the sun's distance may now be ascertained within limits of error not exceeding 100,000 miles on either side of the true distance.

On the whole, Science has every reason to congratulate herself on the results achieved during the observation of the late transit. There was a good deal of blundering at the outset, and there were some points to be regretted in the final arrangements, but the more important mistakes were corrected in good time. In portioning to the different nations the honours due to them, we must assign to some countries special credit for some matters, while in other matters other countries took the lead. America devoted a larger sum to the expenses than any two nations together, and adopted excellent arrangements. Russia provided for the greatest number of stations; in fact, for more than England, America, and France together. Germany alone combined photographic and heliometric operations in both hemispheres. France distinguished herself by occupying the greatest number of difficult island stations in the southern seas. Lastly, to England must be assigned whatever credit is due to the first discussion of the subject, and the promulgation of a programme for the whole scientific world: had this programme been but correct, and had other nations only accepted the parts assigned to them, England would have been as easily first as she was in 1769, and as she may be—who knows?—in 1882.

IV. THE QUESTION OF ORGANIC EVOLUTION.*

WE, in the latter half of the nineteenth century, are fated to be present at one of the most striking revolutions which have ever occurred in the interpretation of the universe. The announcement of the heliocentric theory of the heavens, the discovery of the law of universal gravitation, the overthrow of the phlogistian chemistry, the creation of the science of geology, and the modern doctrine of the conservation of force are, indeed, philosophically, steps of equal value. But probably none of them, not even the first mentioned, has given rise to so much and so intemperate controversy. Forty years ago, when the "Beagle" was prosecuting her voyage of discovery with the young and unknown naturalist Charles Darwin on board, zoology and botany were the quietest, the least exciting of the sciences. Deemed altogether remote from human interests, with scant fame and scanter material rewards to confer upon their votaries, they were sneered at by poets,† and viewed by statesmen, ecclesiastics, and men of business with supreme indifference. Now all is changed: over a large portion of the civilised world, a zoological question is debated with an interest passing into acrimony. "With the exception of ecclesiastico-political topics, no sphere of thought agitates the educated classes of our day so profoundly as the doctrine of descent." "Darwinism is meat and drink to the daily papers, and to the political and theological periodicals." The terrible truth has dawned upon the world that laws, at first deemed applicable solely to "dumb animals"—to "mere brutes"—extend to man. The theory of ferns and flowers, of beetles and butterflies, has become a theory of life. Notions which have been unfortunately entangled with our creeds, intertwined with our earliest education, and which underlie many of our colloquial expressions, are threatened. No wonder that, as Professor Schmidt expresses the matter,—"Since to non-scientific persons the notorious relationship with apes is the alpha and omega of the Doctrine of Descent—since the most confused heads are often most thoroughly convinced of their own pre-eminence—on no subject do we so frequently hear superficial opinions, mostly condemnatory, and all evincing the grossest ignorance."

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* The Doctrine of Descent and Darwinism. By OSCAR SCHMIDT. London: H. S. King and Co.

Evolution and the Origin of Life. By H. CHARLTON BASTIAN, M.D., F.R.S., &c. London: Macmillan and Co.

† For instance, Ebenezer Elliot, the Corn-law Rhymer.

But whilst the new doctrine has met with so hostile a reception from those ignorant of the organic sciences, if not of science altogether, or at least dominated by a radically unscientific spirit, its welcome among candid, working naturalists, capable of understanding the evidence on which it rests, is equally remarkable. Agassiz, one of its bitterest opponents, though his own researches had helped to prepare its advent, exclaimed almost passionately that he did not expect to see so many of the best minds of the age range themselves under the standard of Evolutionism. To use an expression of Professor Schmidt's: "Scales fell, as it were, from the eyes of fellow-labourers and spectators, and the rapidity with which the theory makes its way affords the best evidence that it took shape, and was proclaimed at the proper moment." Its effect upon the sciences concerned has been the best argument in its favour. Stray facts, unintelligible, or at least unaccountable, observations, at once arrange themselves in definite order, like well-disciplined soldiers at the tap of the drum. Zoology and botany have received a purpose which before they lacked. Best of all, the necessity of verifying the "Darwinian" doctrine has produced and is still producing a plentiful harvest of discoveries. The works of Evolutionists are not barren apologetics for a stationary creed, but successful applications of a vital principle. Different thinkers may attach greater or less importance to "natural selection," the power by which, according to Darwin, the mutation of species, is mainly effected; other powers, modifying, controlling, and extending, may and probably will be brought to light. But the doctrine of evolution itself, we believe, has scarcely failed to receive the assent of any conscientious and qualified naturalist who has given it a fair trial. "The proof of the pudding," says a homely old adage, "is in the eating," and the proof of a theory, in the same manner, is in the working.

Professor Schmidt points out that, although the honour of this great discovery belongs incontestably to Mr. Darwin, yet previous thinkers had been turning their inquiries in the same direction. Of these, the foremost place, perhaps, belongs to Lamarck, who, in the earlier part of the century, was loudly shrieked over by the Cuvierian school (then dominant). As early as 1804, in his "Philosophie Zoologique," he enunciated views which, had they been worked out in detail, and verified by carefully selected evidence, would have almost anticipated Darwin. He held that our systematic definitions and gradations are purely artificial; that nature has produced neither orders, families, genera, nor even immutable species. He regards, as the main cause of divergence from the original type, peculiarities acquired by the efforts of animals to accommodate themselves to a change in external circumstances, and the use or disuse of organs—causes which certainly cannot have been operative in the vegetable world. Of our great comparative anatomist, Richard Owen—another herald of the avatar of Evolution—Professor Schmidt remarks: "From the ichthyosaurus to man he sees the connection of descent; he denies that the influence of circumstances is decisive; he rejects a dozen times any kind of miracle; but the next moment he cleaves to miracle again, namely, to an innate tendency towards a certain future development, not imposed by circumstances and dependent on them, but conducive to a special purpose. Thus deal the trimmers, who through fear of consequences appease their scientific consciences with a word."

The opinion that "Goethe actually proclaimed the Doctrine of Descent, or was, even in a poetical sense, its inspired prophet," though entertained by Haeckel is rejected by Schmidt.

It may prove interesting briefly to examine some of the leading doctrines of the old school, and to note the transformation they have undergone in the light of Evolutionism.

By the old Natural History, species—in contradistinction to variety or sub-species on the one hand, and to sub-genus on the other—was viewed as a something objective, and capable of a rigorous demarcation. For its recognition a twofold criterion was put forward; a certain degree of mutual resemblance or morphological unity in all essential characters, and a kindred connection by the ties of a common descent from a hypothetical ancestral pair. On the faith, chiefly, of one well-known negative instance,* it was assumed that all the individuals of a species—even though belonging to distinct varieties—were capable of producing fertile progeny, whilst sexual intercourse between members of different species resulted either in sterile offspring or in none at all. It is surprising how much was here taken for granted. Both these tests utterly failed in practice. The physiological criterion, baseless as we now know it to be, was neither applied, nor indeed capable of application, in one case in a thousand. Who ever tried whether every two supposed species of some closely connected group of insects were or were not capable of producing prolific offspring? Experiments of this nature, continued for a few generations, can only be carried out upon animals in captivity. Now it is well established that, under such circumstances, certain animals refuse to breed at all. Undoubted varieties have been found which will not propagate with the original race. Thus "the variety evolved in Paraguay from our domestic cat pairs no longer with its ancestral stock, nor does the tame European guinea-pig with the wild ancestral stock of Brazil." Where, again, is the proof that the forms of dogs, known to be mutually reproductive, are all sprung from one common ancestry? The balance of evidence rather shows "that in various parts of the world, and at various periods, wild species of the genus Canis have been domesticated, of which the crosses produce fertile progeny to an extent almost unlimited." The orthodox zoologists of the schools of Linnæus and Cuvier thus reasoned in a circle, proclaiming that "forms belong to the same species, because they may be fruitfully crossed, and because they may be fruitfully crossed they belong to the same species; and, on the other hand, because such and such forms, when crossed, produce no fertile progeny, they constitute different species, and because they are different species, they generate no fertile offspring."

Nor was the morphological test more happy. To decide what variations of form indicated mere "sports," sub-species, and varieties, and what, on the other hand, were grave and persistent enough to constitute species, was left to the tact or the prepossessions of each systematist. The results were as discordant as might be expected. Thus, taking the true

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* That of the mule. It has since been established that the hare and the rabbit, two species quite as distinct as the horse and the ass, are capable of producing offspring which, after many generations, show no decline in fertility. Among birds, fruitful hybrids are known to be produced both amongst the warblers and the gallinaceous tribe.

Geotrupidæ found in Britain, some entomologists resolve them into ten species, and others only into two.

Pages might be filled with cases where certain forms have been classified as distinct species, till the discovery of a series of intermediate individuals has reduced them to one. Haeckel points out that the calcareous sponges may be either comprised under one species, or, with equal justice, divided into 591. Certain characteristics are supposed sufficient to prove specific diversity among the true cats,* whilst in the genus homo more essential features are held, merely to indicate "varieties" or "races."

The stern logic of facts, therefore, compels us to admit that "species" is not capable of rigid definition; that between it and "race," or "variety," there is no absolute demarcation, the one passing by insensible gradations into the other; the one as well as the other signifying merely a group of beings, simultaneous or successive, which resemble each other more closely than they resemble anything else. Yet this ill-defined something the old school call upon us to admit as, in its essence, immutable!

It is still urged that vague, indefinable, and subjective as the idea of a species may be, we have never witnessed one species actually transmuted into another. This objection is, however, little better than irrational. An ephemeron may never have witnessed the evolution of the frog from the tadpole. Is the change the less real on that account? When we shall be able to compare series of specimens, anatomical preparations, and carefully-executed photographs of animal species extending at least over a few thousand years, it will be quite early enough to raise this point. To draw any conclusion from the identity of the domestic animals as preserved in Egyptian mummies or figured on Egyptian monuments, with the same species as now found in modern Europe, is presumptuous in the extreme.

But the question may be retorted: who has ever known any organic being produced by the alternative method, a direct and immediate act of special creation? The stone record tells us of species that have disappeared, and of others that sprung up in their stead. Have the latter arisen in full maturity from the ground, or been condensed from the air, fitted up by a *Deus quidam deceptor,* with Mr. Gosse's ὀμφαλος? The individual animal is evolved according to unvarying law from antecedent animals, at first as a simple and rudimentary cell, but gradually becoming differentiated and individuated as a complex and separate being. Does not analogy indicate that such, too, must have been the origin of the groups we call species? Astronomy, physics, chemistry, can dispense with miracle; zoology and botany must prepare to do the same if they are to be recognised as sciences at all.

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* The domestic cat occasionally exhibits all the range of colour and design found in the genus Felis, exclusive of the Lynxes. We find concolorous individuals reminding us of the lion, puma, and black tiger; others with vertical, tiger-like stripes; others, again, with the cloudings of the ocelot, and others with bands of spots closely resembling those of the leopard. If irregular crossing were prevented, there can be little doubt but each of these types could be distinctly developed.

Turning from general considerations to individual cases, let us take the following evidence:—"In the whole series of strata," says Hilgendorf, "the varieties of *planorbis multiformis* are distributed in such a manner that individual layers are characterised as successive strata, by the exclusive recurrence or by the predominance of single or several varieties, which, within the layer, remain constant, or slightly variable, but towards the limits of the next layer, lead by transitions towards the succeeding forms. The intermediate layers furnish evidence that the other forms originated by gradual metamorphosis from the earlier ones; they, moreover, render it possible to range form to form, and to trace the evolution backwards; hence it becomes manifest that, what above seemed distinctly divided meets below." "The forms diverge so greatly," adds Professor Schmidt in comment, "and are so constant in the main zones, which tell of periods of repose, that in accordance with old conchological practice, they would be unreservedly claimed as species *if the connecting links were not too conspicuous.*"

Finding such connecting-links—such plain evidence of continuous evolution in case of animals which, like the shell-bearing mollusca, have a characteristic portion of their structure composed of somewhat permanent materials—is it too much if we infer that, were the stone record undamaged, and were all animals equally imperishable, a like continuity would meet us in every group?

Turning to species still existent:—"Who that has read the comprehensive investigations of H. Müller can doubt that the honey-bee, as it gradually attained its bodily advantages and peculiarities, developed likewise the higher mental powers, corresponding with the more minute and complex organism of her brain?" Or, again, look at the Ancon sheep—a mere variety, commonly so called by those who refuse to see that between species and variety no valid line of separation can be drawn. Yet had this mere variety been placed in sole occupation of some country where it might have multiplied undisturbed, there is no doubt that in the course of ages it would have reached a point at which it would no longer have been capable of productive intercourse with the original stock, as we have seen is the case with the tame guinea-pig. Let us then suppose that its *habitat* had been visited by Cuvier and a band of his disciples. Unless specially informed of its origin, would not these sages calmly note it down as a "good species?"

The origin of parasites, internal and external, has long been a stumbling block to naturalists. Were these beings created simultaneously with the species they infest, or have they been produced by some subsequent miracle? Such were the questions asked by the old school. It was admitted that these vermin are not now found living otherwise than parasitically; that their present structure unfits them for an independent life, and that many of them are peculiar to some one particular animal, or even to some part of an animal. They were the *bètes noires* of teleologists, none of whom, we believe, has yet made the tape-worm or the Trichina the subject of an extra Bridgewater Treatise. But in the light of the doctrine of evolution the difficulty vanishes. In successive generations these animals have become gradually modified to the places they inhabit and the parts they play; so that to trace what were their original forms and attributes may now be beyond the reach of the human intellect. Do not similar considerations—the gradual modification of certain low organisms, animal or vegetable, or, perhaps, doubtful—explain the origin of such diseases as rabies, syphilis, and variola, which in our days never occur except in consequence of transmission from one animal to another, and which we cannot conceive as primordial?

We are thus naturally led to one of the cardinal doctrines of the old Natural History. Who of us entomologists, when curiously examining some specimen, has not been accosted, perhaps by a child, perhaps by a grown-up outsider, with the question: "What is the use of that creature, and to what end was it made?" On our replying, as might often be the case, that to the best of our belief and knowledge it was of no use at all, but decidedly pernicious, have we not been told that "every living thing was made for some good end?" This doctrine is somewhat inconsistent in the mouths of sparrow- and rook-shooting farmers, aphis and red-spider-poisoning gardeners, and a very large majority of the human race. It is, too, decidedly obscure. Are all animals useful to man, or to the universe at large? If we take the former view, we shall generally have to strike a balance between the good and ill deeds of a species, and shall very often find the latter preponderate. Even the Rev. J. G. Wood confesses himself unable to "make out a case" in favour of the earwig. If we attempt to prove all species beneficial to the universe at large, our difficulties become still greater. But the new Natural History solves these enigmas. It shows us that an animal exists, not because it is beneficial to man, or because it is an integral and necessary, though minute, part of some vast mechanism, but because it is in harmony with the conditions in which it is placed. The house-fly lives and multiplies, neither to scavenge our dwellings, nor to propagate disease by conveying putrescent and morbific matter to our food and our persons, but because it so far has come off victorious in the struggle for existence. The burying-beetle, which is a far better scavenger, and which does not distribute zymotic poisons, is rare, and becomes rarer, because the conditions under which it can multiply are less easily met with.

Another doctrine of the old school, now in the course of rapid decomposition, is that the fauna and flora of every district are perfectly adapted to the climate, soil, and other conditions of that district. So far is this view from according with facts, that we very often find a foreign plant or animal, on introduction into a country, flourish with such luxuriance as to interfere with, and even extirpate, certain native species. That such should be the case is perfectly conceivable to a Darwinian, and as perfectly inconceivable to the opponents of the doctrine of Evolution.

Professor Schmidt is one of the most consistent and thorough-going of his school. Like Messrs. Darwin and Haeckel, but unlike Messrs. Wallace, Henslow, Murphy, &c., he does not stop short at the anthropoid apes, but embraces man also in his deductions. But he differs from Darwin at the opposite end of the series—the first beginning of life. Here we would wish, without advocating, briefly to expound his views. He quotes from Du Bois-Reymond this passage: "It is once for all incomprehensible how, to a mass of molecules of nitrogen, oxygen, carbon, hydrogen, phosphorus, and so on, it can be otherwise than indifferent how they lie or move; here, therefore, is the other limit to the knowledge of natural science—the former limit being the incomprehensibility of matter and motion. Whether the two limits to natural science are not, perchance, identical, it is, moreover, impossible to determine." Professor Schmidt then adds: "In these last words the possibility is indicated that consciousness may be an attribute of matter, or may appertain to the nature of atoms. And we may add that the attempt has of late been repeatedly made to generalise the sensory process, and to demonstrate it to be the universal characteristic of matter, as by von Zöllner, in his work on the Nature of Comets. . . . We must either renounce the possibility of comprehending the phenomena of sensation in the organism, or 'hypothetically add to the universal attributes of matter, one which would cause the simplest and most elementary operations of nature to be combined, in the same ratio, with a process of sensation.'" In the lower animals the phenomena of life assume a more vegetal character; and in many groups of lower beings, which Haeckel has recently comprised under the name protista, we see the processes of metamorphosis of tissue, nutrition, and reproduction taking place indeed, but in a manner so simple and undifferentiated, that we must attribute to these beings a neutral position between plants and animals. We gain the conviction that the roots of the vegetal and animal kingdoms are not completely sundered; but, to continue the simile, merge imperceptibly into each other by means of a connective tissue. In this intermediate kingdom the much derided primordial slime of the natural philosophers* has regained its honourable position. Many thousand cubic miles of the sea-bottom consist of a slime or mud, composed in part of manifestly earthy inorganic portions, in part of peculiarly formed chalk corpuscles; and finally, which is the main point, of an albuminous substance which is alive. This living slime, the so-called Bathybius, does not even exhibit individuality, or the definiteness of a separate existence; it resembles the amorphous mineral substances, each part of which bears the characteristics of the whole. . . . . It is only with great exertion that we are able to accommodate ourselves to the idea of a living mass, either absolutely formless and undefined, or defined arbitrarily and accidentally."

We are far from desiring to encounter speculations like the above with ridicule. But before they are accepted they must undergo a verification, which must be left as a difficult and splendid task for posterity.

We are thus naturally brought face to face with the question to which Dr. Bastian has long devoted his attention, the Origin of Life.

It was once supposed, both by the learned and the vulgar, that animals were occasionally produced without parents, in the way known as "equivocal" or "spontaneous" generation. Virgil's recipe for producing a swarm of bees out of the carcase of a bull is a curious relic of this belief—the more valuable from the light it throws on the habits of thought prevalent among men of culture in the palmiest days of classic antiquity. In the dark ages, insects, reptiles, and the like, were supposed to originate spontaneously from putrefying organic matter and mud, or to be called into existence by the malice of witches. Rösel, in his quaint "Insekten-Belustigungen" and Swammerdam, in the "Biblia Naturæ," were at great pains in refuting this view by tracing beetles, butterflies, &c., from the egg, through the larva and pupa to the perfect state. By degrees the doctrine "*omne vivum ex ovo*" was established as the orthodox confession of the scientific world. Even the lowest types of animal and vegetable life were proclaimed, on experimental evidence, incapable of originating except from pre-existing germs, no matter how favourable the circumstances. Professor Lister declares that "the doctrine of equivocal generation has been chased successively to lower and lower stations in the world of organised beings, as our means of investigation have improved." Mr. Justice Grove, in his presidential address at the meeting of the British Association, in 1866, observes that, "if some apparent exceptions still exist, they are of the lowest and simplest forms." The experiments of Pasteur, who has taken up this question more in the spirit of a political or theological partizan than that of a philosopher, are generally supposed conclusive as against the doctrine of abiogenesis. The doctrine of Descent has led to a re-examination of the question. It must not, indeed, be supposed that a belief in Evolution necessarily implies the admission of equivocal generation. Darwin himself, as we have stated above, seems to contend that, though organic life being once enkindled upon our globe, its development and extension follow according to natural laws, yet that its origin may have been due to the direct intervention of creative power—in other words, to miracle. Sir W. Thomson derives the first rudiments of life from "a moss covered fragment of another world,"—a solution, or rather evasion, of the difficulty, the weakness of which has not escaped the notice of anti-evolutionists. Professor Huxley, whilst denying that life ever originates from lifeless matter *at present*, holds that were it given him "to look beyond the abyss of geologically recorded time," he might then indeed "be a witness to the evolution of living protoplasm from non-living matter." These admissions show that, consistently or not, abiogenesis is not the universal creed of evolutionists. On the other hand, it must be conceded that, if equivocal generation is once demonstrated, the opponents of Darwinism have no longer a cause to defend. Heterogenesis—the transformation of organisms already existing—is conceivable without abiogenesis, but abiogenesis necessarily involves heterogenesis.

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* The German "Naturphilosophen," as applied to a certain school of thinkers, does not correspond to our "natural philosophers." Physiophilosophers, or philosophers of nature, might be the best rendering.

Under these circumstances it is highly desirable that the question should receive the fullest attention, and be decided, if capable of decision, in a manner which will admit of no appeal.

Towards this final solution, Dr. Bastian furnishes an important contribution in the work before us. He describes the following experiments, which we are bound to pronounce most significant.

Experiment I.—A strong infusion of turnip was rendered faintly alkaline by *liquor potassæ*, and to this a few separate muscular fibres of a cod-fish were added. Some of this mixture was introduced into a flask of nearly two ounces capacity. Its neck was drawn out and afterwards hermetically sealed by the blowpipe flame, while the fluid within was boiling. When thus closed the flask was about half full of fluid. It was then introduced into a digester, which was gradually heated, and afterwards kept at a temperature of 270° to 275° F. for twenty minutes, though it seems well to point out that, if we include the time taken for the water of the digester (in which the closed flask was immersed) to attain this heat, and also again to cool down to 230° F.; this flask was exposed to temperatures above 230° F. for one hour, as I myself carefully noted at the time. When withdrawn from the digester the closed flask was kept at a temperature of 70° to 80° F. for eight weeks, and during part of this time it was exposed to direct sunlight. After it had been ascertained that the flask was free from all crack or fault, its neck was broken in order that its contents might be examined. The reaction of the fluid was found to have become decidedly acid, and it had a sour though not fœtid odour. The fluid was very slightly turbid, and there was a well-marked sediment, consisting of reddish fragments and a light flocculent deposit. On microscopical examination the fragments were found to be portions of altered muscular fibre, whilst the flocculent deposit was composed for the most part of granular aggregations of Bacteria. In the portions of fluid and of deposit which were examined, there were thousands of Bacteria of most diverse shapes and sizes, either separate or aggregated into flakes. There were also a large number of monilated chains of various lengths, of a kind frequently met with in abscesses and other situations where pyæmia or low typhoid states of the system exist in the human subject. There where, in addition, a large number of *Torula* corpuscles, as well as of brownish nucleated spore-like bodies, gradually increasing in size from mere specks, about 1-30,000th up to 1-2500th of an inch in diameter. Lastly, there was a small quantity of a mycelial *Fungus* filament, bearing short lateral branches, most of which were capped by a single spore-like body."

Here, therefore, both animal and vegetal organisms make their appearance. The second experiment is not less conclusive :—

"A strong infusion of common cress, to which a few of the leaves and stalks of the plant were added, was enclosed in a hermetically-sealed flask in the same way, heated in the digester at the same time (and therefore to the same temperature), and was subsequently exposed to the influence of the same conditions as already mentioned in connection with the last experiment. This flask was, however, opened one week later. Before breaking the neck of the flask, the inbending of the glass under the blowpipe flame showed that it was still hermetically sealed. The reaction of the fluid was distinctly acid, though there was no notable odour. The fluid itself was tolerably clear and free from scum, but there was a dirty-looking flocculent at the bottom of the flask. On microscopical examination much altered chlorophyll existed, either dispersed or aggregated amongst the other granular matter of the sediment, and amongst this three minute and delicate *Protamœbæ* were seen, varying in form, and creeping with moderately rapid slug-like movements. In the same drop of fluid more than a dozen very active monads were seen, each provided with a long, rapidly-moving lash. There were also several unjointed *Bacteria*, presenting most rapid progressive movements. Many *Torula* corpuscles and other fungus spores also existed, as well as portions of a mycelial filament, containing equal segments of colourless protoplasm within its thin investing membrane."

We especially call attention to the following sequel :—

"A drop of the fluid containing several of these active monads was placed for about five minutes on a glass slip in a water-oven, maintained at a temperature of 140° F. *All the movements of the monads ceased from this time, and they never afterwards showed any signs of life.*"

These experiments, which, as the author informs us, "are merely two selected from several others in which even higher temperatures, were originally had recourse to in order to free the fluids and flasks generally from anything like a trace of living matter," are very important. If we would deny that in such cases life is produced, not *ex ovo*, only two courses appear to be open. We must either—which Dr. Bastian himself calls "the shortest way out of the difficulty"—deny the facts, or we must, in the face of experiment, maintain that low organic forms, or their germs, can sustain a temperature of 275° F. without injury. This, as far as we are aware, has not yet been accomplished. On the contrary, numerous direct experiments prove that *Bacteria* and their germs "are uniformly killed by an exposure to 140° F. for five minutes." Some authorities "assume that the mere minuteness of the germs of *Bacteria* may serve to protect them from that destructive influence which heat exerts upon living matter generally." This doctrine, we need hardly say, has not the shadow of a fact in its favour. Not less baseless is the notion that some germs may escape the destructive action of boiling water by being spurted out of the fluid on to the sides of the glass, when the process of boiling commenced. In a sealed flask this is utterly out of the question, since every part of the interior is acted on by the steam. As to the assumed "protective action of lumps," we fail to see its validity. No matter how badly any kind of organic matter may conduct heat, a tiny particle placed in a relatively large quantity of boiling water, and kept for above an hour at temperatures exceeding 212 F°., must be heated through and through to a point utterly inconsistent with the maintenance of organic life.

As to the statements made by travellers concerning living insects found in hot wells, we quite agree with Max Schultze and Cohn that they ought to be received with extreme caution. But even if literally correct, they prove nothing. The animals found in such situations have been specially trained to withstand extraordinary temperatures by a process of natural selection. But the germs of *Bacteria*, &c., about which Dr. Bastian argues, have undergone no such training. The author calls attention to a very interesting fact, that the very process which Pasteur devised for the better preservation of wines, is based on the fact that at temperatures of about 140° F. (60° C.) organic germs and spores, ferments, &c., are destroyed!

Dr. Bastian deserves great credit for the manner in which he has narrowed the issue. Unless we can detect some fatal flaw in his experimental evidence, we must, perforce, admit that abiogenesis is not merely possible, but that as regards the lowest forms of organic life, it is a matter of daily occurrence.

We must, however, point out that, upon the first dawn of life on our globe, these experiments, interesting as they are, throw no light whatsoever. They all involve the use of matter, which, though in itself lifeless, is the result of antecedent life. Such matter in the primordial world is, by hypothesis, absent. To solve the question, it will be necessary to prove the spontaneous development of life in purely inorganic matter.

In Professor Schmidt's work we notice a curious misstatement, which we can only attribute to an oversight in transcription. We read :—" If throughout the great family of the Coleoptera, genera and species are to be found with imperfect flying apparatus, consolidated wing-covers, &c.,—if the *whole family of the Staphylinæ does not possess the organs of flight*, no one dreams of considering them as arrested forms." Now the fact is, that the majority of the Staphylinidæ have large and powerful wings, and use them sometimes too well for our comfort. Many of the smaller species fly into our eyes in still, warm, autumnal days, and cause exquisite pain by their habit of erecting their bristly abdomen.

Another curious error occurs on p. 53, where a diagram is described as representing the larva of the " great black beetle (*Hydrophilus piceus*)." The *Hydrophilus piceus* might be called a " great black water-beetle," but the term " blackbeetle " is in England unfortunately applied to the cockroach (*Blatta orientalis*), which is no beetle at all, but an orthopterous insect.

The great merit of Professor Schmidt is, that he enables the English public to become acquainted with the results of Haeckel, Wagner, Nageli, Graber, and other German naturalists, who have so ably and industriously applied the doctrines of Darwin in actual zoological research. His work will be further useful as an able exposure of the shadow-fighting of some of the most prominent anti-Darwinians. " A renowned zoologist," we read, " one of the few who adhere to the old belief, has taken the useless trouble of proving that the skull of the orang could not possibly be transformed into the human head. As if the doctrine of Descent had ever asserted such nonsense !" The *buffos* of the platform, the press, and the pulpit, are here told—what they ought to have ascertained previously—that " man does not stand in the direct line of development from the age. The cheap jest, produced with so much glee, of inquiring why we do not behold the interesting spectacle of the transformation of a chimpanzee into a man, or conversely of a man by retrogression into an orang, merely testifies the crudest ignorance of the doctrine of Descent."

We cannot help expressing our regret that England and Germany are left to work out this great question alone. Is there no place for France, once the home of philosophic zoology—the country of Buffon and Lamarck, of St. Hilaire and of Blainville, the "Cuvierivorous?" Is her "official science" fatally smitten with alethophobia?

V. SELENOGRAPHY: ITS PAST, PRESENT, AND FUTURE.

By E. Neison, F.R.A.S., &c., &c.

SELENOGRAPHY, or the study of the moon's surface, may be said to have been commenced by Hevelius, the celebrated observer of Dantzic; for, though earlier astronomers had examined the moon, and there had been even prior to the discovery of the telescope much speculation as to the nature of the surface, yet to Hevelius are due the first systematic observations of our satellite's surface. When Hevelius commenced his study of the details of the moon's surface, the subject was practically new; Galileo had already, it is true, discovered the moon's libration in latitude, and understood how a diurnal or parallactic libration must also ensue, though it is doubtful whether, with his means, it could have been detected; but his lunar observations, like those of Scheiner and others, were too imperfect to be of any value. A primary and important result of the observations made at Dantzic was the discovery of the libration in longitude, which from his far more accurate observation did not escape Hevelius, and which he explained from the circumstance that the moon from its uniform rotation on its axis always presented the same face to the centre of its orbit, while the earth was situated at one of the foci. Considering the optical means then at the disposal of Hevelius, his telescope not magnifying more than forty diameters, the completeness and general accuracy of his map shows the great care and success with which his observations were made, and for many years it remained the best chart of the moon existing. As is well known, he adopted as a method of nomenclature, a system based on the fancied resemblance between the lunar and terrestrial formations; whilst from estimating the distance within the dark portion of the moon, that the summit of the peaks remained visible, he computed that some of the lunar mountains must be 17,000 feet high—a very corect estimate of probably the Apennines, the mountains measured.

Four years after the publication of the "Selenographa" of Hevelius, in 1647, appeared the "Almagest" of Riccioli, of Bologna, containing a lunar map, less perfect on a whole than that of Hevelius, but still a very creditable result; and in it was introduced a much improved system of nomenclature, where the names of distinguished astronomers and mathematicians were introduced in place of the feeble terrestial analogies of Hevelius. On the Continent this new system met with general acceptance, and has, since the date of Schröter's observation, entirely superseded the earlier, though, until the close of the last century, it was still employed in England.

Newton turned his attention to the subject of the lunar librations, and in a letter to Mersenius, in 1675, amended Hevelius's explanation of the cause of the lunar libration in longitude, by showing it was a necessary sequence of the uniform rotation of the moon on its axis, in the same time as a complete revolution in its orbit, combined with the variable motion of the moon in its orbit; but, like Hevelius, considered the axis of rotation to be perpendicular to the plane of the ecliptic. Later, Newton also showed that, as a consequence of the earth's attraction and the coincidence of its period of revolution and rotation, the moon must be elongated in a direction passing through the centre of the earth when in mean libration, which was the cause of the moon always presenting the same hemisphere to the earth. From this spheroidal form of the moon, Newton pointed out a real libration of the moon must ensue from the retardation exerted by the earth's attraction to the longer axis of the spheroid, passing from a line directed to the earth's centre, though compelled to do so by the libration in longitude, causing thus a vibratory motion in the direction of this longer axis.

Selenography received its next advance from the great astronomer Dominic Cassini, who, by investigating the known conditions affecting the lunar observations, and making many new observations during a period of twenty-two years, announced in 1693 his complete theoretical solution of the problem of the lunar libration, and showed the peculiar relation between the nodes of the moon's equator and orbit on the ecliptic; so that planes through the moon's centre, parallel to the planes of its equator, orbit, and ecliptic, intersect one another on the same straight line, and the two former always on opposite sides of the third—the ascending node of the lunar equator being thus coincident with the descending node of the moon's orbit, both on the ecliptic. Cassini deduced from his observation the value 2° 30′ for the inclination of the lunar equator to the ecliptic, and a mean value of 5° 0′ for the inclination of the orbit to the ecliptic, thus completing a work of the very highest importance to selenography.

Cassini also published in 1680 a small map of the moon, some 20 inches in diameter, containing the results of his earlier observations, but, like its predecessors, only founded on eye estimates of the positions of the spots; whilst though, from his superior optical means, it was more complete, than Hevelius's, it was on a whole not more accurate, and for a considerable period remained little known.

In 1748 Tobias Mayer, the celebrated mathematician and astronomer of Gottingen, as part of his researches on the moon, investigated the problem of the lunar librations, and by determining the difference in right ascension and declination between the apparent centre of the moon and the lunar spots Manilius, Dionysius, and Censorinus, obtained material for ascertaining how far Dominic Cassini's theory of libration agreed with observations. By the middle of 1849 Mayer had obtained 27 measures of Manilius, 9 of Dionysius, and 12 of Censorinus; and in a memoir in the volume for 1750 of the "Societe Cosmagraphique," of Nuremburg, by employing equations of condition, showed that the inclination of the moon's equator to the ecliptic was 1° 29′, and that sensibly Cassini's theory was correct, though he found a slight difference between the positions of the nodes of the lunar equator and orbit, which he made 3° 45′ apart, a result sufficiently small to indicate that its origin was in the errors of observation.

Mayer had intended to form a complete map of the moon in 25 sections, and with this view made a series of 47 additional measures of 21 lunar spots, besides those already mentioned; whilst he determined the position of 63 other formations by careful estimates, in which he excelled, of their distance from the measured objects. From the few measures of each spot made, the positions of all except 8 were only approximate, the average error being about one degree. Pressure of other engagements, especially his lunar tables, and his early death in 1762, prevented his carrying out his intentions, but in 1775, amongst his "Opera Inedita," was published a small map 8 inches in diameter, founded on his measures, which remained for many years the best lunar map, and was employed by Schröter.

From a consideration of careful micrometrical measures of the position of Manilius on three occasions, in October, 1763, Lalande obtained for the value of the inclination of the moon's equator to the ecliptic 1° 43′, which he regarded as exact, though founded on so few measures; whilst he regarded his results as showing the coincidence between the nodes of the lunar equator and orbit, as demanded by Cassini's theory of the moon's librations.

From its interest and importance the question was attacked theoretically by Lagrange, in an important memoir in 1764, where, by an analytical investigation, it was shown that the equality of the periods of the lunar rotation and orbital revolution was a necessary consequence of the earth's attraction; and further pointing out that the earth's influence was capable of producing this equality, even had it not originally existed. Later, in 1780, Lagrange showed that the coincidence between the nodes of the moon's equator and orbit involved in Cassini's theory of its libration was a necessary sequence of the earth's attraction. It also was pointed out by Lagrange that the moon's form must be that of an ellipsoid whose shortest axis was its polar axis and longest axis directed towards the earth's centre.

During the period 1784—1802, Schröter, of Lienthal, in Hanover, with powerful reflecting telescopes, made many observations of the moon's surface, which he was the first to examine in detail, and he obtained what he believed to be proof of the existence of a lunar atmosphere and of active volcanic changes. Though many of Schröter's observations were important, and his drawings, though rough and wanting in detail, are faithful, he took insufficient precautions against changes produced in the appearance of lunar objects by variation of libration and illumination, which were, indeed, then little understood; consequently, many of his supposed changes and evidences of an atmosphere have been shown to be fallacious, and have involved the rest in perhaps undeserved discredit. The results of Schröter's observations were published in his "Selenotopographische Fragmente," the first volume appearing in 1791 and the second in 1802.

About the commencement of the present century, the theoretical results obtained by Lagrange, with respect to the lunar librations, were confirmed by a full investigation of Laplace, who showed, moreover, that the lunar secular inequalities were without influence on the relation between the periods of rotation and orbital revolution, and the nodes of the lunar equator and orbit. Later, the theoretical investigation of this question was completed by Poisson, who showed that some minor inequalities of the second order, affecting the lunar elements neglected by Lagrange, were likewise without influence on the above relations.

As already mentioned, owing to the ellipsoidal form of the moon, a real libration must ensue from the vibratory motion of the longer axis of the moon, due to the endeavour of the earth's attraction to keep it directed towards the earth's centre; whilst the causes producing the moon's optical libration carried it on alternate sides of this direction. Laplace and Poisson both investigated the condition of this real libration, whose amount depended on the mutual relations existing between the lunar axes, and from which it appeared that, though all the inequalities in the true longitude and latitude of the moon must produce an effect on the moon, yet all these would be insensible, except in the case of those inequalities known as the annual equation and the equation of the centre, both of whom might be expected from the probably theoretical form of the moon to produce perhaps sensible real librations in longitude, though the latter could not amount to one-fifth of the former.

At the desire of Laplace, Bouvard and Arago, in 1806, undertook the investigation of this point, to detect, if possible, this theoretical real libration, and made during that year 18 observations of the position of the lunar spot Manilius, but their plans were interrupted by other work. Bouvard, two years after, renewed his work, and between September, 1808, and October, 1810, obtained 124 measures of the position of Manilius. Employing these observations as a basis, Nicollet, in 1816-18, undertook the investigation of the lunar libration, with the view of confirming by observation the theory of libration, and detecting any sensible real libration, from whose amount the form of the moon might be determined. As a result from the 124 observations made by Bouvard during 1808-10, Nicollet showed the distance between the nodes of the lunar equator and orbit on the ecliptic could not exceed 9′ 19″, a quantity so far within the errors of observation as to show its origin was in them, thus affording a complete confirmation of the theory of libration first announced by Cassini, and shown to be theoretically correct by Lagrange, Laplace, and Poisson. From these observations Nicollet also deduced that there existed a real libration of the moon in longitude corresponding to the annual equation in the lunar theory, and amounting to 4′ 46″ of selenographical longitude.

With a view of further improving these interesting and important results, Nicollet made a series of 32 new measures of the position of Manilius during 1819-20, and combining these with the 124 measures formerly employed, and the 18 made by Arago and Bouvard in 1806, he determined that the maximum real libration in selenographical longitude corresponding to the annual equation in the lunar theory amounted to 4′ 49·7″, and the figure of the moon, as deduced from this value, Nicollet showed was inconsistent with the figure of equilibrium it would have possessed had it been originally fluid. Poisson pointed out, however, that the data was insufficient for the rigid determination of this point, and also computed, from Nicollet's results, that the real libration in longitude corresponding to the moon's ecliptic inequality, only amounted at a maximum to 39″, or less than one-fifth of a second of arc.

From the same observations Nicollet deduced the value 1° 28′ 45″ for the inclination of the lunar equator to the ecliptic, and found for the selenographical position of the centre of Manilius latitude 14° 26′ 54″, longitude 8° 46′ 56″.

These results were in the highest degree interesting, and possessed no small importance, but, as remarked by Nicollet, Bouvard's optical means were very small, and it was very desirable that the investigation should be repeated not only with far more powerful instruments, but a greater number of observations be employed.

For the really systematic observation of the lunar surface, with the view of the solution of the many interesting and important questions connected with it, the accurate determination of the position of all the principal formations was essential, as by no other means could the identity of the various objects observed at different times be ascertained, and any changes that might occur be recognised. Hitherto there had, however, existed no data on which to found a trustworthy map of the principal features of the lunar surface; for with the exception of some eight formations whose position had been fixed by Mayer, and the very accurate determination by Nicollet and Bouvard of the place of Manilius—one of them—all the other formations had been laid down by practically eye estimations. Measures of the position of the principal objects upon the moon were therefore requisite for further progress, and then the almost unknown smaller delait could be properly inserted.

In 1824 appeared Lohrmann's "Topographie der Sichtbaren Mondöberflache," comprising four sections of what was intended to be a complete lunar map, $37\frac{1}{2}$ inches in diameter, divided into 25 sections, being the first detailed lunar chart based on scientific principles. As a foundation, employing a method devised by Encke, Lohrmann made a series of 150 good measures with a parallel wire micrometer, on a $4\frac{4}{5}$ inch achromatic, of the distance of 21 spots from the limb, all the objects being within the area of his four sections, namely, from 12° east to 38° west longitude, and 12° south to 37° north latitude. Of these points of the first order, as they were termed, twelve, whose position rested on the results of from 8 to 12 measures, may be regarded as determined with fair accuracy, five based on from five to seven observations were not quite so satisfactory, whilst the remaining five were founded on too few measures to be considered of much value. These measured positions were, however, far too few for the great area that rested on them, and few, if any, auxiliary points were measured. The section of his map, based on these results, was, however, a great improvement on all former charts, and constituted the first trustworthy delineation of the moon's surface; it was therefore unfortunate that failing health prevented its ever being completed. In some points, however, Lohrmann's work was defective, as no height measures were made only those earlier made by Schröter with imperfect means quoted, and few formations had their dimensions measured.

In 1837 appeared Beer and Mädler's "Der Mond," founded on the results of observations made during the years 1830 to 1837, accompanied by a complete lunar chart, in four quadrants, on a scale of $37\frac{1}{2}$ inches to the lunar diameter. As a foundation for their map, Mädler, whom it is understood was the principal observer, made a series of 919 direct micrometrical measures from the limb of the principal lunar points, adopting Encke's system of computing the moon's libration and reducing the measures. Rejecting 104 of these —principally the earlier—as discordant, imperfect, or doubtful, from the remainder, the position of 92 points were determined, 7 being, however, points already chosen by Lohrmann, whilst the position of each point rested on from 8 to 10 measures. In constructing their map, as Beer and Mädler combined Lohrmann's results with their own, they had 105 points of the first order, only two of which rested on under five measures, as well as a considerable number of auxiliary points of the second order.

Mädler's measures were made with a parallel wire position micrometer, with a power of 140 and a field of 14½′, or about 20 revolutions in diameter, the thickness of the wires being 1·91″, but as the edge of the field was imperfect, distances exceeding 14 to 15 revolutions could not be measured direct; consequently, objects more than 10′ or 11′ from the limb were measured from an intermediate spot, generally Posidonius, Piccolomini, Messier, Gassendi, or Seleucus, whose distance from the limb was also measured. To eliminate as much as possible incidental errors, and to simplify the computations, measures were only made when the moon was near the meridian and possessed a greater altitude than 18°. The distances were found by bisecting the spot with one wire and bringing the other tangentially to the limb, a somewhat difficult operation to perform in a right ascension direction, owing to the rapid motion of the moon, and the difficulty of putting the two distant objects in position at the same time. Each result consisted of two measures in a right ascension direction, and one at right angles.

Beer and Mädler considered that from the difficulty of exactly adjusting the wires of the micrometer so as to keep both in position, and from the irregularity of the moon's limb and variability in the amount of irradiation at the edge, the results of a single measure could not be regarded as giving the position of the point within 30′ of longitude or latitude near the centre, and proportionately greater towards the limb, the uncertainty of a single result being about 8″ to 9″, a result borne out by their measures, which, rejecting the unfavourable points, show the uncertainty of a single measure to be 7·2″. From this the average probable error of one of their points of the first order, resting as already mentioned on from 8 to 12 measures, usually might be expected to be from 2·2″ to 1·8″, or 8′ to 7′ of selenographical longitude or latitude near the centre, a result sufficiently approximate to the actual probable errors for the more favourable points. In the above no account has been taken of the considerable personal equation now known to exist in the observations of the lunar limb; and this is a weak place in Encke's method of determining positions of the first order, as employed by Mädler. Of necessity, the position of all the points were measured by Mädler, mainly from one pair of limbs, either S. and W. or S. and E., or else N. and W. or N. and E., and therefore the resulting personality acts as a constant error always in one direction, and cannot be eliminated, whilst escaping consideration in the resulting "probable error." It is true, on some occasions, Mädler measured from opposite limbs, but then always through the aid of an intermediate point, consequently introducing double errors, from having to make two separate measures, whilst these measures were too few to exert much influence. By comparing Lohrmann's measures with Mädler's, the influence of this personal equation, combined with some differences due to the effect of aperture on the irradiation at the limb, is at once revealed, for there exist a mean difference of 23′ in longitude and over 17′ in latitude between the places of the points they have both measured; and this would appear to be too great a difference to be ascribed merely to errors introduced by the few observations made by each of these spots, even were it not noticeable that all Mädler longitudes are nearer the centre of the moon than Lohrmann's, and, with one exception, all his latitudes further north. This would indicate, probably, a personal difference in the method of observing similar to that known to exist in ordinary lunar observations, and it is unfortunate that the few spots observed by both astronomers renders its direct determination impracticable.

Besides measures of points of the first order, Mädler made numerous determinations of the position of points of the second order, as he termed those points of minor importance, whose place he ascertained by measuring their distance and position angle from a line joining two of his points of the first order. This method gives, however, very inferior results to the results of measures of the first order, as, in addition to errors incidental to measurement, the position carry all the errors in the determination of the points of the first order, and when the distance amounts to more than some five or six degrees, is liable to considerable errors of its own. The great majority of these points rest on a single measure, and as their distance from a point of the first order is usually considerable, much uncertainty must attach to their position, which cannot be considered as having a probable error of under three or four times that of the neighbouring point of the first order.

Mädler likewise determined the diameter of 148 of the principal formations, and made 1095 measures of the height of 830 lunar peaks, whilst he also considerably increased and improved the lunar nomenclature, adhering, of course, to Riccioli's system, with Schröter's modifications, distinguishing the smaller objects by Greek and Roman letters attached to the name of the nearest important formation.

Mädler's instrument was a Fraünhofer achromatic of 3¾ inches aperture, and though smaller than Lohrmann's, yet probably of finer quality, its excellence being apparent from its admitting commonly of a power of 300 being used for drawing the lunar details, but its moderate aperture possesses important bearings on many of the results given in Beer and Mädler's "Der Mond," where the influence of its want of capacity for dealing with the more delicate phenomena is often exhibited. There can be no question, however, of the great advance on all its predecessors of the "Mappa Selenographica," not only from the vast amount of detail for the first time shown, but in its general accuracy, owing to its being based on the result of actual measures. Though deficient in minuter detail from the small aperture of their instrument, and though not implicitly to be trusted on the more delicate features, yet in its general fidelity the "Mappa Selenographica" is superior, not only to all its predecessors, but likewise to its successors.

Upon the conclusion of Beer and Mädler's great work, it was generally considered as established that the great questions in connection with the condition of the moon were finally settled, with the exception of the reason why volcanic action had produced such different effects on the moon and earth, and the real origin of some of the more inexplicable formations, principally the great ray or streak systems, and the rills or clefts, whilst it was considered certain that the moon was to all intents an airless, waterless, lifeless, unchangeable desert. With this opinion prevalent, the natural effects of such great works as Beer and Mädler's shortly ensued; the attention of astronomers was directed to other fields, and selenography, resting on its laurels, made no further progress for many years; and until 1865 Schmidt, at Athens, may be regarded as the sole selenographical worker on an adequate scale.

The only point in connection with lunar physics that during this period excited attention was that of the real libration of the moon, and was due to the investigation of Bessel. In a memoir in the "Ast Nach," in 1839, he entered into an examination of the question of the moon's real libration, as presented by the theoretical investigations of Lagrange, Laplace, and Poisson, and pointing out the uncertainty attending the value deduced by Nicollet. As a basis for further investigation, he deduced the position of the crater Mösting A by two careful measures with the Konigsberg heliometer. This preliminary examination was carried on by Dr. Wichmann, in 1847-48, who, after a careful investigation of the problem to be determined, applied the results of 50 measures with the Konigsberg heliometer, in 1844-46, to deduce the real libration. The results obtained by him were still more inconclusive than those arrived at by Nicollet; Wichmann's conclusion being that the real libration of short period could not exceed 10′ of selenographical longitude, and probably not 7′; but as to its exact amount, the observations failed to afford any satisfactory information.

Though Wichmann's individual measures were considerably superior to those of Bouvard and Nicollet, yet their small number, only 44 separate results, and the short period of 14 months in which they were made, with the irregular interval between them, renders them inferior as a series for this purpose to those of Bouvard and Nicollet, whose 174 measures were spread over a period of 4 years, and, commenced in 1806, extend to 1820. The uncertainty with respect to the amount of the real libration of the moon remains, therefore, as great as ever. The value obtained by Wichmann for the inclination of the lunar equator to the ecliptic was 1° 32′ 9″, or over 3′ greater than resulted from Nicollet's investigation; and this discrepancy between the results obtained by the two investigators adds much to the uncertainty attending the whole question. Incidental to his investigation, Wichmann found for the position of Mösting A, longitude 5° 13′ 23′,′ latitude 3° 10′ 55″, and that Burckhardt's value of the moon's semi-diameter was considerably too small. The determination of the problem of the real libration of the moon still required to be performed, and as Wichmann observed, it must be based on a considerable number of observations; not less than two hundred at regular intervals appears suitable, and should extend over at least three years.

Schmidt devoted his attention mainly to the production of a lunar map that should adequately show the true details of the lunar surface which he recognised Mädler's failed to do, though representing most faithfully the greater formations. For this purpose he determined that a map 75 inches in diameter would be the smallest adequate size, being four times the area of Mädler's, though the results show even this to be too small for this purpose, producing too much crowding to be desirable. During his work, Schmidt has made over a thousand drawings, and three times as many height measures—but it is understood few (if any) measures of position of the first order, but based his map entirely on the result of Mädler and Lohrmann.

In 1865, when the deficiency of Beer and Mädler's map in the minuter detail had become more generally recognised, the British Association appointed a committee for the purpose of making further progress in the mapping and cataloguing of the lunar surface, and the result was the production of a new system of nomenclature, the commencement of a complete lunar catalogue, and the construction of four sections of a preliminary outline map on a scale of 200 inches to the moon's diameter. After three years' work, however, the committee was no longer reappointed, and the map and catalogue remained in an unfinished condition, making the very slowest progress.

Through both these means, though mainly through the labours of Schmidt, much new lunar detail has been observed, sketched, and mapped; but little progress has been made in a most important branch, namely position measures of the lunar formations, though these are absolutely essential for further progress. Mädler's results were insufficient even for the proper delineation of the "Mappa Selenographica," and are entirely inadequate for the foundation of a larger map like Schmidt's of four times the area, much less for one like the proposed final British Association map of over seven times as large, or its outline map, which has nearly thirty times as great an area; therefore, for the proper drawing of these charts; and any smaller than the proposed final map of the British Association—namely 100 inches to the moon's diameter—would be inadequate; more positions of the first order must be made if they are in any way to be trustworthy. To supply this want should therefore be the first object of further lunar work.

In October, 1866, Schmidt noticed that the lunar crater, Linne, described by Mädler, with whom Lohrmann is in accordance, as a deep crater 5½ miles in diameter, was no longer visible, and in November he looked equally in vain, and announced, therefore, that Linne had disappeared. This attracted at once the attention of astronomers, and numerous observations were made; but no trace of the crater, as described by Lohrmann, and Beer and Mädler, could be detected, though soon after a very minute crater cone was found to exist on the whitish spot occupying the site of Linne. Since then the crater seems to have widened, and the remains of a crater of dimensions nearly equal to those described by Beer and Mädler have been detected: therefore it is generally considered that no real change has occurred, though no explanation can be given of the appearance as seen by Lohrmann, and Beer and Mädler. The importance of measures of the principal lunar formations was, however, apparent; for had not Lohrmann and Mädler measured Linne, it is doubtful whether it would have received the attention merited on Schmidt's announcement; whilst had they and Schmidt measured carefully the dimensions of the crater, no uncertainty could have existed as to its real appearance. Schmidt and others have since pointed out cases of apparent change, but being in unmeasured points, have received little elucidation. During the period 1869-71, many observations were made of the relative visibility of the minute crater cones, white spots, and streaks on the surface of the walled plain Plato, and the results have been discussed under the direction of a British Association Committee, whose reports were published in 1871 and 1872, where it was shown that the minute objects on Plato underwent curious and inexplicable variations in visibility; whilst the whole floor showed an apparent darkening of marked extent during each lunation. Numerous observations have also been made, indicating peculiar changes in the visibility of different lunar regions, not dependent on variation of illumination; many new formations have been detected, now so conspicuous as to appear hardly capable of being overlooked by earlier astronomers, were they then as distinct; changes in tint and colour have been recognised; and finally, considerable differences have been noticed in the position of the light streaks from Mädler's drawings, whilst a number of new ones have been seen. The result, in short, of the observations of the last period of selenographical activity has been to reopen all the questions nearly previously considered settled by Beer and Mädler, before the true nature of the lunar details were generally understood, and with regard to which the small aperture of their telescope placed them under serious disadvantages.

The present condition of selenography is essentially, therefore, one of uncertainty; and though the earlier astronomers have by their work cleared the ground, and defined the questions to be solved, their solution is still to be accomplished, and for this end further observations are necessary to afford the requisite data.

The principal points to which this further micrometrical work should be directed are essentially the following:—The determination of an adequate number of lunar positions, comprising those of all the principal formations; the measurement of the dimensions of the principal craters and of the slopes of the walls of the chief classes of lunar formations, as well as the relative levels of the principal mares or great plains, both important features, with regard to which no data exists; and the determination of the dimensions under different illuminations of some of the peculiar white spots occupying at full the position of various lunar formations. Means should also be taken for a re-determination of the apparent brightness of the different portions of the lunar surface, and of their colour; for good reasons appear to exist for believing distinct changes have occurred since Beer and Mädler's time, and it should be remembered that such changes were suspected by Mädler himself. Measures of the height of many of the walled peaks, and of the depths of the chief formations, are especially valuable, after the apparent disappearance of several of Mädler's deep craters, although the many unpublished measures of Schmidt will be, when accessible, a great addition to our present data on this subject.

Measures of the lunar diameter, and observations of occultations of stars present themselves as highly important for the determination of the question of the existence of a lunar atmosphere, such as is at present indicated by the known apparent disagreement between the telescopic and occultation semi-diameter. Measures of the shadow length of different ring-plains at different periods might reasonably be expected to afford some indications of the presence of a denser local atmosphere, due to local causes. By determining the distance between some conspicuous lunar point and a star before occultation, evidence of the strongest kind might be obtained with regard to whether a lunar atmosphere exists, though at present no suitable lunar point near the limb has had its position sufficiently well determined to be available for this purpose.

Much, likewise, remains to be done in the observation of the details of the surface, and the careful construction from these of a trustworthy map of them. Based on sketches made at intervals during a period of thirty-five years, and unaided by the results of systematic measures of their positions, Schmidt's map, when issued, which is unfortunately not likely to be for a considerable period, will, though of the highest value, add materially to the lunar work requiring to be done. It is certain to require much revision from its very nature, and must give rise to many uncertainties requiring settlement, whilst, as it differs in many points from Beer and Mädler's, whose general accuracy is of the highest, these discrepancies will need most careful investigation, a work necessarily of considerable labour. Moreover, it appears certain that the position of numbers of the formation will need alteration, when their true places have been ascertained by systematic measures. Being founded likewise on drawings made at different epochs, some uncertainty must attach to the combination of these into a single connected whole, whilst, as is known, a considerable personality attaches to lunar observation, one observer detecting certain classes of objects far readier than others. Much also remains to be done in the proper combination of the isolated details seen at different times into a connected whole, and here appears one of the most difficult selenographical labours. For on the moon, the details of the surface being nearly only rendered visible when thrown in relief by shadow, it is rarely that an entire formation is seen equally distinct at the same time, but nearly always the details come out little by little, those first seen disappearing as the newer come into view; so that it is necessary, from isolated observations, to piece together, as it were, the details into a connected whole. Considering the great difficulty in studying these details, and the fact that much is only visible under conditions when the rest is quite invisible, while some are rarely, if ever, seen at all, the great labour and difficulty in piecing all together so as to obtain the nature of the formation which connects together all the isolated detail seen is apparent; whilst it must be remembered that, owing to the effects of libration, the appearance of these details are constantly altering. Lohrmann, and Beer and Mädler, have seldom attempted to do this, but have merely mapped down the object seen; and a similar course seems to have been followed in the main by Schmidt; but the study of the real nature of the surface will necessitate its being carried out in full, if we are ever to properly comprehend the meaning of what is visible on our satellite.

So far no reference has heen made to the results obtained by the employment of photography in selenographical investigation, though many fine photographs of the moon have been obtained mainly through the labours of De la Rue and Rutherfurd. Hitherto, however, the results obtained through the aid of photography have been of less value than was generally anticipated, for though for many purposes the photographs have been and will continue to be of great service, with regard to the two principal ends, it was trusted they would be of the greatest value: they have failed to realise expectation. In so far from affording unerring representations of the real condition of the lunar surface, enabling its details to be examined with ease at leisure, they have hitherto proved entirely inadequate for this purpose, even the finest photographs exhibiting hardly a trace of this detail, but show only the coarser features of the larger formations and the mere existence of the smaller, whilst even what is to be seen appears at times, from differences in actinic and illuminating power, so different to its actual appearance as to render it recognisable only with difficulty. Those already obtained seem to be deficient in sensitiveness to slight variations in illumination, which prevents the smaller detail being separately distinguishable, but blurs the whole into one general mass, which no mere enlargement can rectify, and it is doubtful whether this fault can be overcome, except by taking a larger image directly, even if that proves successful. Whatever may be the cause, the result has been to render lunar photographs of little assistance to the study of the details of the surface of the moon, as Mädler has already pointed out.

The other respect in which much was anticipated from lunar photographs was in the accurate mapping of the surface of the moon, as it was considered they would afford a means by which the true positions of the lunar formations might be laid down by means of a series of measures. In this, however, they have hitherto been of little service, only having been employed by Birt to lay down secondary positions on the lunar map of the British Association, by whom they were also employed to give the approximate dimensions of the principal formations. For these purposes, were the number of well-determined points much increased, they might prove of considerable assistance, and more use made of them for this end than has been. But for the determination of points of the first order with any accuracy, they are far inferior to direct measures, and the results neither satisfactory nor sufficiently trustworthy perhaps. Even were there no uncertainty introduced by the want of sharp definition, or by variations of illumination, all certainty is lost in the errors that can be, or perhaps must be, introduced, when dealing with such delicate material as collodion film and paper. And it appears impossible to guard against the effect of the vagaries of such materials as collodion films to the desired amount of accuracy, and the use of daguerreotypes seems impracticable, considering the large scale necessary.

With regard to classes of measured points on the lunar surface, on which depend our power of constructing a trustworthy map on an adequate scale of the surface of our satellite, they should consist of standard points, and points of the first, second, and third orders. The accuracy of the first should be sufficiently great to enable all the others to be measured direct from them: the points of the first order would be employed for the same purposes as the present members of this class, whilst the points of the third order would comprise all those auxiliary objects whose, at least, approximate position must be known for the proper construction of the outlines of the larger formations, and to serve as nuclei round which group the smaller not measurable details.

In measuring points upon the moon, the limb is considerably inferior as the origin of measures to any small conspicuous object on the surface, such as a small bright craterlet, not only from its irregularity and the irradiation accompanying it, but also from the considerable personal equation known to exist in observing it. To eliminate as far as possible the effects of these, if measured from the limb it should be from all four, which would considerably weaken the above disadvantages. When practicable, however, far more accurate results might be expected by measuring from some of the smaller lunar formations, preferably one of the bright lunar craterlets, such as are equally distinct in high and low illumination. These objects would constitute the first class, or standard point, and should rest on the results of from 40 to 50 measures, so as to reduce the probable error of the position to a minute amount, and so render them available as points from which to measure the rest. The members of the second class, which were termed by Mädler points of the first order, should be founded on from 10 to 15, or, in the more important, 20 measures, so as to increase their accuracy, and would be generally measured from the last, unless, as might be in some special cases, the limb offered superior advantages. The third class, or points of the second order, as here termed, would consist of objects whose place had been determined from 5 to 8 measures from points of the first class, and therefore probably be as accurately known as most of Mädler's points of the first order, and could be employed alone or in conjunction with one of the last class, to determine points of the third order within a convenient distance from them. The formations included in the third class would consist of the points of the second order of Mädler, and comprise all the principal smaller lunar formations, whose position was necessary for the proper mapping of the surface, or else possessed some special importance or interest of their own. Measured in an analogous method to Mädler's, they should preferably rest on two separate measures from different points, and as the increase in the number of available points would enable their distances from these to be much reduced from what Mädler was obliged to use, their accuracy would be probably nearly tripled.

In employing, as a point from which to measure other positions, a lunar spot instead of the limb, a considerable advantage is gained, inasmuch as a comparative great discrepancy between the assumed libration of the moon and the true would produce a scarcely sensible error in the resulting position; whereas, as measured from the limb, any error in the assumed libration produces a corresponding error in the position of the points, and thus, in the former case, the reduction of the measures made is much simplified, an additional advantage of no slight importance. Any connection, moreover, in the assumed position of the standard point that further observations may indicate, would also admit of being applied without trouble sufficiently approximately to the positions deduced from it by measures, unless it was of very considerable magnitude, which would hardly be conceivable under the conditions; and this, likewise, is no slight advantage.

So far, with reference to the measurements for the determination of the position on the moon's surface of the principal lunar objects necessary for the proper construction of a trustworthy map of the moon, and for the further progress of those selenographical observations, requisite for any advancement to be made in the solution of the many problems in connection with our satellite, and in the construction from the visible records of the past history of the companion to the earth.

Intimately connected with the subject of the measurement of the objects upon the moon is that of its librations, and a proper knowledge of the conditions introduced by these into lunar observations is essential to accurate selenographical work. It has been already stated that the moon is subjected to two classes of librations, the greatest, which may be termed the optical libration, being due to the nearly uniform rotation of the moon on its axis, and its variable motion in its orbit, in conjunction with the results of the inclination of the lunar equator and orbit to the ecliptic; whilst the other may be called the real libration, and is due to the actual motion as if it were of the major axis which is directed towards the earth. The optical libration admits without difficulty of being computed for any given time, and may be held to include the parallactic or diurnal libration due to the same cause as the lunar parallax, and depending on the position of the observer with relation to the earth's centre. This libration, therefore, presents no difficulty: but with regard to the real libration of the moon, this is not the case, for there exists no trustworthy determination of its amount; and this introduces some uncertainty into observation depending on the true lunar libration being known, which it is desirable should not be allowed to remain. The existing uncertainty on this point lends additional advantage to the employment of positions on the moon itself, rather than its limb, to measure points on the surface from, as under these conditions a small variation of the true lunar libration from the assumed produces no sensible error. As measured from the limb, the libration must be known correctly, as any error produces a corresponding error in the resulting place; whilst this error cannot be eliminated by increasing the number of observations, but only by spreading them over a very considerable period of time, naturally a highly inconvenient condition for many reasons.

It has been already mentioned, that from the mathematical investigation of Lagrange, Laplace, and Poisson, the complete theoretical conditions of this real libration in so far as results from the theoretical ellipsoidal form of the moon, is known. Its amount depends primarily upon the relation holding between the axes of the lunar ellipsoid, and on this point theory can afford little trustworthy data, considering the imperfect condition of our present knowledge; while they depend in part upon the condition of the initial motions of the moon, which is at present unknown. The amount of the real libration must therefore be deduced from observation, or at least one of the principal periodical inequalities of the moon's axial rotation constituting the real libration must be so found, and from that, by theoretical considerations, the others found. Mention has already been made of the observations commenced by Arago and Bouvard, and finished by Nicollet, with the view of determining directly the amount of this real libration if sensible, and of the success attending Nicollet's research based on these observations, resulting in the deduction of a value of 4′ 49″ for the maximum value of the periodical real libration in longitude corresponding to the annual equation. The results of this investigation are, however, unsatisfactory, the value deduced being very uncertain, owing to the inadequate optical means employed by Bouvard; and Nicollet considered it advisable that the whole research should be repeated with the assistance of more powerful instruments. On these grounds Wichmann, in 1847, made a fresh attempt to determine the amount of real libration of the moon from a series of 44 independent observations, but entirely failed to obtain a satisfactory determination of the amount of real libration, whilst the discrepancies between the results of the two memoirs is additional reason for further investigation.

The results obtained renders a new investigation of this subject very desirable, for it is only by this means that the limb of the moon can be rendered a satisfactory origin of measures for determining the position of the standard points on the surface of the moon, to be afterwards employed to measure other lunar objects from; and it is only the limb that is available for this purpose, whilst the sole standard points already existing are the central mountain of Manilius and the small lunar crater Mösting A. Moreover, the disadvantages of being obliged to employ the lunar limb as an object to be measured, or to measure from, has long been recognised in astronomical work, apart from selenography, where exactness is necessary, as in the determination of accurate longitude from moon culminations. For such purposes it has long been proposed to employ, instead of the somewhat indefinite limb, with its variable irradiation and irregularities, a bright lunar crater, without these disadvantages; but this is inadmissible with the moon's real libration, still only indefinitely known.

In repeating, however, the research on the real librations of the moon, several modifications might be adopted, besides simplifying the preliminary reductions employed by Nicollet and Wichmann, and it might be made after the following plan. Mädler has already pointed out that a small brilliant crater is preferable as a point to be employed to measure to any central mountain, like that of Manilius, used by Bouvard and Nicollet, and has suggested Mösting A and Triesnecker C and B as suitable both in form and from lying close to the centre of the moon, a desirable feature; and the former consequently, at the instance of Bessel, was employed by Wichmann. Of these, Mösting A is alone well suited for this purpose, but a still better is almost the crater Triesnecker A; whilst a third can be selected from one of the three craters, Hipparchus C, G, or E, the last being most favourably placed, and all considered perhaps the most suitable for this object. Three points have been selected; for it would add comparatively little to the work, though much to the accuracy of the results, were the investigation made to depend on three separate series of objects on different sides of the lunar equator, as Poisson has shown; whilst by mutual comparison some of the incidental errors could be eliminated. By means of a series of simultaneous measures from the central mountain of Manilius, a series would be obtained of like nature to the former, but free from the effects of the real libration of the moon and the irradiation at the limb, which would of course be also eliminated as much as possible from the former series by measures being taken from all four directions. By the mutual comparison of the two series, the accuracy of the results of the discussion of the first would be improved, whilst means would be afforded of comparing with the results of Bouvard and Nicollet. The comparison with the investigation of Wichmann would be independently done, as the crater Mösting A would be embraced in both series. Employing suitable instrumental means, the errors due to mere instrumental effects might be reduced considerably from those of Bouvard, and rendered equal, perhaps, to Wichmann, and from a series of from two to four hundred measures a trustworthy conclusion with regard to the real lunar libration might be arrived at.

Attention has now been directed, not only to the present position of selenography, but also to the progressive stages by which it has arrived at the condition it now stands in—a little known history; whilst the direction where further selenographical work appears most requisite, and which should be the first to be supplied, have also been indicated in some detail, and our present knowledge on these points described. The necessity of the introduction into lunar observations of micrometrical measures has been again urged with special stress, for it is only on a foundation of exact micrometrical work that the desirable accuracy in the delineation of the lunar formations can be obtained; for, though practice renders the results of an experienced observer sufficiently faithful, as a rule, for many purposes, yet in the comparison of drawings of the same formation at different epochs, all certainty is lost with regard to differences, unless based on adequate measures, as is exemplified by the results of Linne, and still more so in the case of the twin Pytheas of Mayer, to say nothing of minor instances.

In all branches of science too much faith is put in the results to be achieved by mere piling up of observation on observation, and trusting, by so doing, some fortunate discovery may result; but it cannot be too strongly urged that the hugest mass of isolated observations made without scope or character is far inferior in value to a well-considered series of observations made in pursuance of some definite plan and directed towards some distinct end. The former, it may be said, will serve as a mine from which, by labour, much may hereafter be extracted to confirm or disprove theories that may later be advanced; but even for this purpose they possess little value: for, to search through the mass for those observations that may be useful, with the almost certainty of finding them deficient in some especial feature necessary for them to be of real value in the particular direction wanted, renders it easier to obtain the results required in a different manner. The erection of an elaborate edifice, intended to be enduring upon an uncertain foundation, has long been recognised as unsatisfactory; yet much of this characteristic attaches to many selenographical hypotheses that have, especially of late, been promulgated. The origin of most of these may be ascribed to the vagueness incidental to desultory lunar observations, which, seldom pushed sufficiently far, often, as in other branches of science under similar conditions, afford untrustworthy results. Too much stress, therefore, cannot well be laid on the necessity of systematic series of observations being made, rather than mere desultory work, which seldom afford anything in value adequately representing the time and labour spent.

Above all, it may again be reiterated, stands the necessity so often urged of the completion of the determination of the positions of the principal lunar formations that has, since the time of Beer and Mädler, remained apparently untouched, despite the earnest appeals from the British Association Committees; when this has been successfully carried out, and the foundations for a thorough trustworthy aquaintance with the physical condition of the moon's surface laid, then the detail can be filled in at leisure. Not until this is done can much success in solving the various selenographical questions be expected; but then, in the words of our greatest selenographer, the late Baron von Mädler, "may our satellite, after the monstrous fables which for almost the space of many thousand years have gained credence respecting it, begin, not only by its course, but also by its natural constitution, to permit us to pierce deeper into the secrets of the Fabric of the Universe!"

VI. MODERN ENTOMOLOGY.*

ENTOMOLOGY is, of all sciences, the least fashionable. Insects, indeed, form what may be called the round number of the animal world, all other tribes constituting a mere fractional overplus. They display the most wonderful variety of structure and development. They offer excellent opportunities for studying the origin, the mutations, and the geographical distribution of species. They present the most striking examples of the singular phenomenon of mimetism. Their transformations, their architecture, their intelligence, and their social institutions, are themes practically exhaustless. Among their ranks are included our most formidable enemies—beings far more difficult to deal with than the wolf or the tiger. For all this, few works on the natural history of insects make their appearance—fewer, we believe, now than was the case thirty years ago. Of these few, a large proportion are little more than compilations and manuals, chiefly of British species—to which a large part of our entomologists limit their attention in a spirit possibly patriotic, but certainly unphilosophical.

Another besetting sin of English lovers of insects—indeed, of English naturalists in general—is a proneness to teleology. We have heard it maintained abroad that a work on the organic sciences, free from all reference to final causes, was as rare in England as "unfortified" wine. It is curious how completely the old proverb, "It's an ill wind that blows nobody good," decomposes such speculations. Let us take the following passage from one of the works before us:—"The first insect of which travellers unite in complaining is the hated and dreaded mosquito. In its perfect or winged state it is about as annoying a creature as can be, but then it must be remembered that the traveller is but a casual intruder in the natural domain of the mosquito, and must expect the consequences of his intrusion. Devouring travellers is not the normal occupation of the mosquito; for hundreds of successive generations of them may live and die, and not one of them ever see a human being. Their real object is a beneficent one. In their larval state they live in the water, and feed upon the tiny particles of decaying matter that are too small to be appreciated by the larger aquatic beings; and by devouring them, purify the water, and convert death into life. Even in our ponds at home we are much indebted to the gnat larvæ for saving us from miasma; whilst the vast armies of mosquito larvæ that swarm along the edge of tropical lakes, and feed upon the decaying substances that fall from the herbage of the banks, purify at the same time the water and the atmosphere, and enable human beings to breathe with safety the air in which, without their aid, no animal higher than a reptile could have existed."

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* Insects Abroad: being a Popular Account of Foreign Insects, their Structure, Habits, and Transformation. By the Rev. J. G. WOOD, M.A., F.L.S., &c. London: Longmans, Green, and Co.

On British Wild Flowers considered in Relation to Insects. By Sir J. LUBBOCK, Bart., F.R.S., M.P., &c. London: Macmillan and Co.

To this passage, ably written as it is, many exceptions may be taken. If man is not to intrude into the "natural domain of the mosquito," his choice of a dwelling will be very limited; since, with the exception of sandy and stony deserts, scarcely one-fourth part of the land on our globe is free from these pests. We are not sufficiently well acquainted with the nature and origin of miasma to decide whether the larvæ of mosquitos do really purify the water and the atmosphere. That they feed upon particles of putrescent or putrescible organic matter in the waters is very probable; but will not, in that case, their excrements and their dead bodies be as great a nuisance as their original pabulum? They are found in pestilential districts, which proves that their services in a sanitary point of view are at least questionable. They swarm also in regions free from malaria, and where its existence seems highly improbable, such as Lapland. They are known also to be spreading into parts hitherto free from them, and where no malaria had been met with in their absence. How is it that teleologists judge the "contrivances of Nature" so much more leniently than those of Art? What would they say, for instance, of a city where the scavengers and nightmen were employed occasionally as surgeons, hospital nurses, and provision dealers, so that they might distribute putrescent and infectious matter upon the food, the medecines, and the very persons of the inhabitants? Yet such a case would be precisely similar to that of some of Nature's scavengers—to wit, the common house-fly and its allies. These are one moment feasting upon carrion, excrement, and ulcers; and the next, settling upon food, and upon men and animals. That in this manner they are active propagators of disease is no mere supposition. Which, then, is the real function of the fly, scavenging or the spread of disease? We have no more right to assume the one than the other. Surely our most philosopical as well as most reverent attitude is to dismiss, as beyond our reach, all enquiries as to "why" animals exist.

Another error of entomologists is their tendency to digress into learned, but tedious and utterly irrelevant, disquisitions on the names borne by certain genera or species. This may be interesting to the antiquarian, but it is not entomology. Significant names have generally the fatal defect of being too long for men who cannot hope to exceed three score and ten years of life. We wish that, in this respect at least, our modern naturalists would follow the example of Linnæus.

Turning to the chapter on Ants—beings whose wonderful intelligence makes us overlook their destructive propensities—we quote the following passage as a fair specimen of the author's matter:—"In the various accounts of ant-life which have been narrated by observers, there is often an absolutely startling resemblance to the conduct of human beings. We have heard of ants which make regular slave-hunting expeditions into the territory of less powerful ants, carry off their captives, and make them their servants. We know of ants which build walls and domed roofs. We know of ants which have their milch-kine (aphides and scale-insects), and which tend and guard them as carefully as any dairyman tends his cows. We know of ants which cultivate the ground, keep it clear of weeds, sow the future crop, and when the harvest has come to maturity, get it in just like human beings. In the history which now follows, a new and unexpected phase of human life is found to exist among ants—namely, funeral honours paid to the dead and their burial in the earth." The author then quotes from the "Journal of the Linnæan Society," vol. v., p. 217, a communication from Mrs. L. Hutton, of Sydney. This lady, having killed a number of ants which were stinging her little boy, and having flung the bodies aside, witnessed afterwards the following scene: "I saw a number of the ants surrounding the dead ones. At last four ran off very quickly, and I followed them till I saw them enter a hillock containing an ants' nest. They remained here about five minutes, when a number more came out two by two, and proceeded slowly to the place where their dead companions lay. Here they seemed to wait for something, and presently we saw, coming from the other side near the creek, a number surpassing those I had followed, and halting at the same place. Then two ants took up one of the dead ones and marched off, followed by two others as mourners; then two others entered the procession with a second dead ant, succeeded in the same way by another pair, and so on until all the dead were taken up—a number of (I should think) two hundred bringing up the rear. Following the train I found that the two empty-handed followers relieved their fellows in advance, the latter following behind in the place of those who had relieved them, and thus continuing to alternate from time to time. They stopped at a sandy hillock, where those who marched in the rear of the procession commenced operations by making holes. When a sufficient number of graves had been dug, the dead bodies were laid in them, and those ants which had hitherto stood idle were deputed to cover them in. About six would not stir from their places, and on these the others fell and killed them; whereupon they made a single large pit at a distance from the other graves, into which all the six were put and duly covered up. I had frequent opportunities afterwards of seeing the insects act much in the same manner. If one of the workers, however, were killed, it was buried where it fell, and no friends attended the funeral. The ants buried in state belonged to the soldier caste." Many other interesting facts are given in illustration of the reason, or as some persons will still persist in calling it the "instinct," of ants. Dr. Lincecum, who for more than twelve years studied the proceedings of the Agricultural Ant of Texas (*Myrmica barbata*), and who has given an interesting account of his observations in the "Journal of the Linnæan Society," in 1861, mentions a case where these ants had their nest in an orchard. But after a while the orchard was opened to cattle, who naturally ate the succulent grass-grain which the ants had planted. Finding this to be the case the ants abandoned the orchard, and took to making their plantation in the garden and other spots where the cattle could not disturb them.

It is commonly asserted that man alone is a progressive being, and that all the lower animals remain without improvement at the very point which was occupied by their most remote forefathers. To this view it may be of course objected, that we do not possess accurate accounts of the habits of social animals extending over a sufficient length of time to decide such a question. But let us assume that some species of ant did effect a manifest step in civilisation. Any naturalist who observed the result would suppose that he had seen not something new, but merely something which former entomologists had overlooked, and would thus take the credit to himself instead of awarding it to the ants. It thus becomes very easy to deny that the lower animals are capable of progress.

From the ant to the ant-lion is not an unnatural transition. Of these insects, which have no representatives in England, Mr. Wood gives a very interesting account, taken from Mr. Gosse's "Naturalist's Sojourn in Jamaica." The ant-lion larva, it is said, is capable of existing without food for a long time, one of Mr. Westwood's specimens having lived for six months without any nourishment whatever. This, our author remarks, "is to be expected, as the supply of nourishment is very precarious." But if we turn to his account of the tiger-beetle, whose larva lives on the same kind of prey as the ant-lion, and captures it in a somewhat similar manner, we read:—"All carnivorous creatures require a constant supply of nourishment. The internal fire fed by animal fuel burns fast and fiercely, so that a tiger-beetle larva would die of hunger through a temporary deprivation of food, which would little affect the turnip grub or the cabbage caterpillar." This ill records with the remarks made anent the ant-lion. We have always found that carnivorous animals are better able to bear prolonged abstinence from food than are herbivorous species, whose supply of nourishment is, as a rule, so much less precarious.

The superior beauty of the insects in tropical countries has been frequently insisted upon by travellers, and it has been ascribed to the greater intensity of the solar rays in those latitudes. Against this theory Mr. Crookes has argued with much force in his "Handbook of Dyeing and Calico Printing." But the alleged fact is itself open to question. Our author remarks that some English groups of insects are quite as numerous, as large, and as handsome as their foreign representatives. Mr. Bates asks, very acutely, why, if climate have any direct connection with splendid colouration, do we find the females of so many Brazilian butterflies clad in such dull and sombre attire, whilst the males display an almost dazzling lustre? This one fact must, we think, suffice to overthrow the common notion of beauty as a result of light or of temperature.

There is some curious information concerning the habit of certain Carabidæ, which, when alarmed, eject noisome liquids as a means of defence. Some of these liquids are dark coloured and of evil odour, but not corrosive.

The Brachinides, familiarly called bombardier beetles, emit a fluid so highly volatile, that "when it comes in contact with the air it explodes with a slight report, leaving a cloud of thin smoke." According to Burchell, this liquid, in some of the larger tropical species, burns and stains the hands to such an extent, that the capture of the insect requires considerable resolution. The common *Cychrus rostratus* projects, without any report, a drop of colourless liquid, which burns the skin, and which we suspect to be formic acid in a very concentrated state. On this subject there is room for much interesting micro-chemical research.

The existence of blind species of insects is a fact which leads to certain difficult questions. The author describes a blind beetle, *Leptoderus sericeus*, found, as far as is known, only in the caverns of Carinthia, which are inhabited; also of a blind spider (Obisium). Several ants also are blind, including several of the terrible Ecitons, in which family we recognise a curious instance of serial degradation as regards the organs of sight. *Eciton legionis* and *prædator* are not blind, but, as Mr. Bates informs us, have eyes consisting each of a single lens, instead of the compound structure usual in insects. *E. crassicornis* has eyes sunk in rather deep sockets, and always avoids the light, moving in concealment under leaves. If obliged to cross a clear space, it constructs a covered way or tunnel with grains of earth, as do the equally blind soldiers and workers among the Termites. *Eciton vastator* has no eyes, although "the collapsed sockets are plainly visible," whilst in *Eciton erratica* both sockets and eyes have disappeared, leaving only a faint ring to mark the place where eyes are normally situated. It is curious that these totally blind species construct a covered way on coming into the open, proving that in some unknown manner they are aware of the presence of light. The question then arises, are these species blind, *ab initio*, being adapted to dark, subterranean abodes, or have they become blind by a process of gradual transformation? To us the facts of the case seem neither favourable to original "adaptation" nor to "natural selection," since, though eyes may in such situations be rarely or never of use, it is hard to see how their possession should be any inconvenience. Other species, which lead underground lives, are well known to be furnished with eyes. It is quite conceivable, that in the course of many successive generations, the eyes should be atrophied by disuse.

Where different geological formations meet, there the most interesting minerals are often found. Where land and water come in contact, there organic life is generally the richest and most varied. In an analogous manner, the boundary between two sciences often proves a fertile field for research. This is evinced in a striking manner in Sir J. Lubbock's work, which calls attention to a class of phenomena that have, till lately, escaped common observation.

It is familiarly known that plants in general, and flowers in particular, are necessary to the existence of a large proportion of the insect world. Honey constitutes the food of probably all butterflies and moths when in their mature state; of many bees, both social and solitary; of a multitude of Diptera, and even of certain beetles, such as the pretty black and yellow-banded *Trichius fasciatus*, which, from its colour, its general hairiness, and its habit of haunting flowers, is often mistaken for a bee.

But it has been, till lately, little suspected that the interdependence is mutual, and that without the visits of insects, a vast number of flowers would be incapable of fertilisation, and would consequently become extinct. Few persons are ignorant that flowers are the reproductive organs of plants; indeed, we are sometimes afraid lest the prurient prudishness of modern times may pronounce them an offence to delicacy, and hand them over to some self-constituted authority for "suppression." In some species—in analogy to animals—the male and female flowers are respectively assigned to different individuals. Thus all the specimens of *Aucuba Japonica* existing in England happened to be females, and they consequently never produced their brilliant scarlet berries, till a male tree was brought from Japan by Mr. Robert Fortune. In other cases—as in the cucumber and the vegetable marrow—there are male and female flowers on every plant. Lastly, in the majority of species, each flower is hermaphroditic, containing both the male organs or anthers, and the female, or pistil. In all these cases the question arises, how is the pollen needful for fecundation to be transported from the anthers to the pistil? In some cases this is effected by the wind. But in other instances the structure of the flower renders this utterly impossible. Even in the case where the stamens and pistil are in the same flower, there is what seems very like an elaborate arrangement to prevent self-fertilisation. Sometimes the structure of the parts renders this result practically impossible. Sometimes the anthers and pistil do not come to maturity at the same time. There is, therefore, need for some special agency to transport the pollen from one flower to the pistil of the other flowers of the same species, and thus ensure the fecundation of the seed. This purpose is effected by insects, which, flying from flower to flower, convey the pollen adhering to their bodies. Attention must here be called to an important fact, first pointed out by Mr. Darwin, that when a flower is so constructed as to be capable of fertilisation by the wind, it *never has a gaily-coloured corolla*. If a momentary digression may be allowed, we would urge that an observer capable of detecting so capital a point, after it had escaped the notice of generations of botanists, is evidently not the mere amateur theorist which he is represented by the "rash envy" of MM. Milne-Edwards and Quatrefages. It appears, then, that the beauty of flowers, as well as their odours, are not unessential attributes designed for the amusement of man, but subserve an important purpose in the life of a plant. The more conspicuous a flower by colour or scent, the more certainly it will be visited by insects, the more surely its seed will be fecundated, and it will thus be enabled to perpetuate itself. Here, then, we see "natural selection" at work, and in this case it really tends to the preservation and multiplication of the most beautiful, and to a continued increase in brilliance of hue and delicacy of perfume.

Sir J. Lubbock has proved by experiments, which, however, he does not here detail, that insects—bees at least—are really attracted by and can distinguish colours. This, we must remark, is a most interesting fact, as proving a certain community between them and ourselves, not merely in the process of vision, but in the mental faculties. Animals, too, have their æsthetics. But the author continues: "Flowers, however sweet-smelling or beautiful, would not be visited by insects unless they had some inducements more substantial to offer. These advantages are the pollen and the honey; although it has been suggested that some flowers beguile insects by holding out the expectation of honey which does not really exist, just as some animals repel their enemies by resembling other species which are either dangerous or disagreeable."

That such false pretences may really exist becomes all the more probable if we remember that certain plants attract insects by a carrion-like smell, and then kill and eat them. Of these carnivorous plants we find an interesting account in the present work, taken from the observations of Ellis, Hooker, and Canby. If it be asked why the self-fructification of plants should not be desirable, Sir J. Lubbock gives the reply: "Kolreuter speaks with astonishment of the *statura portentosa* of some plants thus raised by him; indeed, says Mr. Darwin, 'all experimenters have been struck with the wonderful vigour, height, size, tenacity of life, precocity, and hardiness of their hybrid productions.'

Mr. Darwin himself, however, was, I believe, the first to show that, if a flower be fertilised by pollen from a different plant, the seedlings so produced are much stronger than if the plant be fertilised by its own pollen. I have had the advantage of seeing several of these experiments, and the difference is certainly most striking. For instance, six crossed and six self-fertilised seeds of *Ipomæa purpurea* were grown in pairs on opposite sides of the same pots; the former reached the height of 7 feet, while the others were on an average only 5 feet 4 inches. The first also flowered more profusely. It is, moreover, remarkable that in many cases plants are themselves more fertile if supplied with pollen from a different flower, a different variety, or even, as it would appear in some instances (in the passion-flower for instance) from a different species. Nay, in some cases, pollen has no effect whatever, unless transferred to a different flower. Fritz Müller has recorded some species in which pollen, if placed on the stigma of the same flower, has not only no more effect than so much inorganic dust, but, which is perhaps even more extraordinary, in others he states that the pollen placed on the stigma of its own flower acted on it like a poison. This he noticed in several species: the flower faded and fell off; the pollen grains themselves and the stigma in contact with them shrivelled up, turned brown, and decayed; while other flowers on the same branch, which were not so treated, retained their freshness."

It may be urged that the fertilisation of flowers by means of insects is a process liable to be frustrated by a number of accidents. Thus a bee or a butterfly may go from one flower to another of a totally different order, when the pollen it conveys will of course be wasted. Sir J. Lubbock enumerates, however, several species of bees which confine themselves to particular flowers. Thus *Andrena florea* visits no other flower save that of *Bryonia dioica*, whilst *Macropis labiata* restricts itself to *Lysimachia vulgaris*.

Why certain flowers are fecundated by bees, and others chiefly by moths, is an unexplained question. In some cases the long proboscis of a sphinx may reach into the tubes of flowers where no bee could penetrate. It would almost seem that bees are more generally attracted by colour, and moths by odour. Every entomologist knows that vinegar, porter, or rum—all strong smelling liquids—is a necessary ingredient in the mixture used in "sugaring" for moths. From the wonderful development of their antennæ, we should infer them to possess the sense of smell in singular perfection.

It must be remarked that by far the majority of flower-haunting insects are hairy, a circumstance which enables the pollen to adhere more readily to their limbs and bodies. The gradation in this respect from Prosopis, through Sphecodes and Nomada to Andrena, Osmia, and Anthrophora, and finally, to the true Humble-bees, is described and illustrated in this work. A somewhat parallel arrangement is pointed out in those flowers which are fertilised by the agency of the wind, without the mediation of insects. In these, such as the alder, the hop, and the wheat, the stigma is branched or hairy, so that particles of pollen borne along by the wind may more certainly adhere. We find here some interesting facts which go to prove that bees, in their quest for honey, are not actuated by some unvarying instinct, but both vary individually in intelligence and are capable of modifying their operations. H. Müller "watched a female humble bee (*Bombus terrestris*) examining an Aquilegia; she made several vain attempts to suck the honey, but after a while, having apparently satisfied herself that she was unable so to do, bit a hole through the corolla. Having thus secured the honey she visited several other flowers, biting holes through them without making any attempt to suck them first—conscious, apparently, that she was unable to do so. . . . Any one who has watched bees in greenhouses will see that they are neither confined by instinct to special flowers, nor do they visit all flowers indiscriminately. It would also appear that individual bees differ in their way of treating flowers. Some humble-bees suck the honey of the French-bean and scarlet-runner in the legitimate manner, while others cut a hole in the tube and thus reach it, so to say, surreptitiously. Dr. Ogle has observed that the same bee always proceeded in the same manner, some always by the mouth of the flower, and some always by cutting a hole. He particularly mentions that this was the case with bees of one and the same species, and infers, therefore, that the different individuals differ from each other in their degrees of intelligence."

Into the admirable exposition of the successive modifications which the mouths of insects have undergone, so as to profit by their visits to flowers, space does not permit us to enter.

Of both the works before us we feel bound, in conclusion, to express a favourable opinion. Sir J. Lubbock, whose treatise forms not the least interesting member of the valuable "Nature Series," points the way to a region where patient research cannot fail to be amply rewarded.

The Rev. J. G. Wood, though we cannot altogether agree with his speculations, is himself a patient and acute original observer, of rare merit, and tells his frequently wondrous tale in the happiest manner. The result is a work as fascinating as the well-known volumes of Kirby and Spence. The illustrations represent no fewer than six hundred species of insects, not copied at second-hand from the works of others, but all drawn from actual specimens.

VII. AËRIAL LOCOMOTION:

PETTIGREW *versus* MAREY.

By Professor COUGHTRIE.

THE great interest taken in aërial locomotion, and the increasing belief in the feasibility of a flying machine, invest works on natural and artificial flight with a certain significance and importance which cannot be over estimated in the present day, characterised as it is by unusual progress and invention.

The works to which we wish more especially to direct attention, and which have attracted an unusual share of notice, are those of Dr. J. Bell Pettigrew, of Edinburgh, and Professor E. J. Marey, of Paris.

The names of Dr. Pettigrew and Professor Marey are well known in the scientific world, and require only to be mentioned. Both gentlemen are physiologists of a high order, both have experimented largely on the subject under consideration, and both, as a consequence, are entitled to be heard.

The object of the present article is to show that these *savants*, notwithstanding certain apparent differences (and notwithstanding much that has been written to the contrary), essentially agree. The fundamental features of flight, according to both, are the same. If there be differences, they refer, for the most part, to time and the mode of treatment adopted, Dr. Pettigrew having published his views some two years before Professor Marey.

Dr. Pettigrew obtained his results by transfixing the abdomen of insects with a fine needle, and watching the wings vibrate against a dark background, by causing dragonflies, butterflies, blowflies, wasps, bees, beetles, &c., to fly in a large bell jar, one side of which was turned to the light, the other side being rendered opaque by dark pigment; by throwing young pigeons and birds from the hand into the air for the first time; by repeated observation of the flight of tame and wild birds; by stiffening, by tying up, and by removing portions of the wings of insects and birds; by an analysis of the movements of the travelling surfaces of quadrupeds, amphibia, and fishes; by the application of artificial fins, flippers, tails and wings, to the water and air; and by repeated dissections of all the parts, directly and indirectly, connected with flight.

Professor Marey obtained his results by gilding the extremities and margins of the wings of the insect with minute portions of gold leaf; by the application of the different parts (tip and anterior margin) of the wing of the insect to a smoked cylinder rotating at a given speed, the wing being made to record its own movements; by the captive and free flight of birds, which carried on and between their wings an apparatus which, by the aid of electricity, registered the movements of the wings on a smoked surface, travelling, at a known speed, in a horizontal direction; and by the employment of an artificial wing, constructed on the plan recommended by Borelli, Chabrier, Straus-Durckheim, Girard, and others.

The treatises on flight and cognate subjects by Dr. Pettigrew and Professor Marey are so elaborate and so profusely illustrated,* that a digest of them cannot fail to be interesting to the general reader, the more especially as in that digest we hope to state in a few words, and in something like chronological order, not only the great leading features of flight, but also the points wherein Dr. Pettigrew agrees with and differs from Professor Marey—these not being generally known.

The parts of Dr. Pettigrew's and of Professor Marey's works which interest us most are those which deal with aërial locomotion and the flight of the insect and bird.

Professor Marey, in his recent book,† describes the figure-of-8 movements made by the wing in space, and for these he claims, and in some journals has obtained, considerable *cudos*, although it is difficult to understand on what grounds.

There can be no question of the fact, that the figure-of-8 movements made by the wing in flight *were first observed*,

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* Dr. Pettigrew's memoirs alone contain over 200 original figures—those of Professor Marey considerably over 100.

† Animal Mechanism: A Treatise on Terrestrial and Aërial Locomotion. By E. J. Marey, Professor at the College of France, and Member of the Academy of Medicine. Henry S. King and Co. 1874.

described, and *delineated* by Dr. Pettigrew, and to this physiologist undoubtedly belongs the high merit of first discovering the true principles of flight.

Dr. Pettigrew published his discovery in the early part of 1867,* and Professor Marey did not write upon the subject of flight till the end of 1868.† There is, therefore, an interval of nearly *two years* in favour of Dr. Pettigrew.

We think it right to draw attention to this circumstance, because Professor Marey does scant justice to Dr. Pettigrew, and because we detect in all Professor Marey's writings on flight traces of Dr. Pettigrew's original discovery. This remark applies equally to Professor Marey's theory and practice of flight.

We hope to be able to prove the validity of our position, as we advance, by a series of parallel passages. The history of science demands that this course should be taken. We begin with the figure-of-8 itself.

Professor Marey, in a letter addressed to the French Academy of Sciences, admitted Dr. Pettigrew's claim to priority in the matter of the figure-of-8 movements made by the wing in space in the following terms :—

> " I have ascertained that, in reality, Mr. Pettigrew has seen before me, and represented in his memoir‡ the figure-of-8 track made by the wing of the insect, and that the optic method to which I had recourse is almost identical with his . . . .
>
> " I hasten to satisfy this legitimate demand, and I leave entirely to Mr. Pettigrew the priority over me relatively to the question, as restricted." (Comptes Rendus, May 16th, 1870, p. 1093).

Since writing the above, Professor Marey has evidently been changing his views; for in his new work (" Animal Mechanism," p. 187) he states that, " notwithstanding this apparent agreement, our theory, and that of Dr. Pettigrew, differ materially from each other."

We have searched diligently for the points of *disagreement,* and find them trifling in character and few in number. The points of *agreement,* on the other hand, are numerous and important.

Dr. Pettigrew, in his letter of " reclamation " to the French Academy,|| to which the foregoing, by Professor

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* " On the Various Modes of Flight in Relation to Aëronautics." Proceedings of the Royal Institution of Great Britain, March 22, 1867.

" On the Mechanical Appliances by which Flight is attained in the Animal Kingdom." Trans. Linn. Soc., vol. xxvi. (Read June 6th and 20th, 1867).

† Comptes Rendus. Tome lxvii., No. 26, p. 1341. Dec. 28, 1868.

‡ " On the Mechanical Appliances by which Flight is attained in the Animal Kingdom." By J. Bell Pettigrew, M.D., F.R.S. Trans. Linn. Soc., vol. xxvi. (Read to Linn. Soc. on June 6th and 20th, 1867).

|| Comptes Rendus. April, 1870.

Marey, is the reply, claims to have been the first to describe and illustrate the following:—

> "1. That quadrupeds walk, and fishes swim, and insects, bats, and birds fly, by *figure-of*-8 movements."
>
> "2. That the flipper of the sea bear, the swimming wing of the penguin, and the wing of the insect, bat, and bird, are screws *structurally*, and resemble the blade of an ordinary screw propeller."
>
> "3. That these organs are screws *functionally*, from their twisting and untwisting, and from their rotating in the direction of their length, when they are made to oscillate."
>
> "4. That they have a reciprocating action, and *reverse their planes* more or less completely at every stroke."
>
> "5. That the wing describes a *figure-of*-8 *track in space*, when the flying animal is artificially fixed."
>
> "6. That the wing, when the flying animal is progressing at a high speed in a horizontal direction, describes a *looped* and then a *waved track*, from the fact that the figure of 8 is gradually opened out or unravelled as the animal advances."
>
> "7. That the *wing acts after the manner of a boy's kite*," both 'during the *down*' and the '*up*' strokes.*

Such are briefly Dr. Pettigrew's views; and if we compare what Professor Marey has written on flight with what Dr. Pettigrew here enunciates, we shall find the coincidences (to use no stronger terms) very striking.

Take the following passages from Professor Marey's recent work as examples:—

> "If we gild a large portion of the upper surface of a wasp's wing, taking precautions that the gold leaf should be limited to this surface only, we see that the animal placed in the sun's rays *gives the figure-of*-8 with a very unequal intensity in the two halves of the image. . . . . It is evident that the cause of the phenomenon is to be found in *a change in the plane of the wing*, and consequently in the incidence of the solar rays. . . . . We shall find in the employment of the graphic method new proofs of changes *in the plane of the wing* during flight. . . . . [In this and other quotations the italics are ours.] It is therefore not necessary to look for special muscular actions to produce changes in the *plane of the wing*; these in their turn will give us the key to the oblique *curvilinear movements which produce the figure-of*-8 *course* followed by the insect's wing."—("Animal Mechanism," pp. 188, 197).

In the passages here cited, Professor Marey admits, not only that the wing of the insect makes *a figure-of*-8 *track in space*, but also that the figure-of-8 is produced by *a change of plane in the wing*.

This is an important admission, for Professor Marey copies at page 201 of his book a figure-of-8 representation from Dr. Pettigrew's 1867 memoir,† in which this change of plane is delineated, and states that the arrows in Dr. Pettigrew's figure all point in one direction, and are wrongly placed. This is a glaring inaccuracy on the part of Professor Marey.

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* "On the Physiology of Wings." By J. Bell Pettigrew, M.D., F.R.S. Trans. Roy. Soc. of Edinburgh, vol. xxvi., p. 332.

† Marey's figure is "Fig. 86, Trajectory of the Wing," p. 201. Pettigrew's figure is at p. 233. Trans. Linn. Soc., 1867., vol. xxvi.

He has in the first place reversed the direction of the arrows in Dr. Pettigrew's figure, and in the second place he makes the half of the figure represent the whole. In Dr. Pettigrew's original figure the arrows are pointing from left to right; whereas in Professor Marey's copy of it, they are pointing from right to left.

In the description given of Dr. Pettigrew's figure, it is distinctly stated that *in extension* the arrows of the figure-of-8 are directed from left to right, but that *in flexion* they are directed from right to left.*

In one complete revolution of the wing, therefore, according to Dr. Pettigrew, the arrows are directed alternately from left to right, and from right to left, and this is precisely what happens in every figure-of-8 delineated by Professor Marey.

Dr. Pettigrew, when speaking of the change of plane occurring during the down and up strokes of the wing of the insect, states that:—

> "A figure-of-8 compressed laterally, and placed obliquely with its long axis running from left to right of the spectator, represents the movement in question.
>
> "The *down* and *up strokes*, as will be seen from this account, *cross each other*, the wing smiting the air during its descent from above, as in the bird and bat, and during its ascent from below, as in the flying fish and boy's kite."†

A little further on, and on the same page of his 1867 memoir, in which the figure-of-8 and waved tracks made by the wing in stationary and progressive flight are delineated, Dr. Pettigrew says:—

> "The figure-of-8 action of the wing explains how an insect or bird may fix itself in the air, the backward-and-forward reciprocating action of the pinion affording support, but no propulsion. In these instances the backward and forward strokes are made to counterbalance each other. . . . . Although the figure-of-8 represents with considerable fidelity the twisting of the wing upon its axis during extension and flexion, when the insect is playing its wings before an object, or still better when it is artificially fixed; it is otherwise when the down stroke is added, and the insect is fairly on the wing, and progressing rapidly.
>
> "In this case the wing, in virtue of its being carried forward by the body in motion, describes an undulating or spiral course."‡

The figure-of-8 and undulating wave movements originally described and figured by Dr. Pettigrew, in March and June,

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* According to Dr. Pettigrew *extension* in the insect signifies "the carrying of the wing in a forward direction, away from the body; *flexion* meaning the reverse, or the drawing of the wing from before, backwards towards the body."—(Trans. Linn. Soc., vol. xxvi., p. 226).

† On the Mechanical Appliances by which Flight is Attained in the Animal Kingdom.

‡ Trans. Linn. Soc., vol. xxvi., p. 233.

1867, have been reproduced by Professor Marey in a variety of forms since December, 1868. They are reproduced in a collective form in Professor Marey's work already referred to, published in 1874.

The importance of the figure-of-8 and wave movements cannot be over estimated, and no one appears to be more keenly alive to their value than Professor Marey himself. When speaking of the figure-of-8 made by the wing in space, originally discovered by Dr. Pettigrew by the aid of the optical method, Professor Marey remarks:—

> "We have seen, when treating of the mechanism of insect flight, that the fundamental experiment was that which revealed to us the course of the point of the wing throughout each of its revolutions. Our knowledge of the mechanism of flight naturally flowed, if we may so say, from this first notion."*

Professor Marey here admits that his knowledge of flight is derived from the figure-of-8 revealed by the optical method; but he admitted, as already stated to the French Academy of Sciences, in May, 1870, that the optical method to which he had recourse was nearly identical with that which Dr. Pettigrew employed, and that in reality Dr. Pettigrew had seen before him, and delineated the figure-of-8 track made by the wing of the insect in flight.

If, however, Dr. Pettigrew was the first to observe, describe, and delineate the figure-of-8 made by the wing in space; and if, as Professor Marey states, his knowledge of the mechanism of flight "naturally flowed . . . from this first notion," then it is quite evident, even according to Professor Marey's own showing, that the discovery of the true principles of flight was made by Dr. Pettigrew, and *not by him*. This follows as an inevitable sequence.

It is easy to extend a discovery once made, but the true discoverer is he who first describes and delineates the fundamental principle, and in the present instance that is unquestionably Dr. Pettigrew.

Dr. Pettigrew not only described and delineated the figure-of-8 and waved track made by the wing in space; he also described and figured the several changes of plane occurring in the wing during an entire revolution.

To him, moreover, is to be traced the important discovery of the *torsion* and *forward action* of the wing both during the down and the up strokes. The torsion and forward action of the wing are indispensable in flight.

The body in flight is *dragged* forward, not pushed forward;

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* Animal Mechanism, p. 234.

but unless the wings themselves fly forward in curves, both during the down and up strokes, as Dr. Pettigrew explains, the body cannot be transmitted from one point to another. Dr. Pettigrew's experiments with natural and artificial wings are quite decisive on this point, as we have ourselves verified.

Dr. Pettigrew was likewise the first to describe and figure *the ellipse* formed by the wing of the bird, and to point out the difference in the direction of the stroke in the wing of the bird and insect, the stroke in the insect being, as a rule, *nearly horizontal*, that in the bird *nearly vertical*.*

Professor Marey, in his first paper on flight, communicated to the French Academy of Sciences,† delineates the wings of the wasp as making *vertical* figure-of-8 loops. Now this never happens in the wasp. The figure-of-8 loops made by the wing of the wasp, as Dr. Pettigrew has shown, are so oblique as to be *nearly horizontal*.

Professor Marey, in his latest work, has corrected this mistake‡, and has delineated the horizontal figure-of-8 loops made by the wing of the insect in a figure nearly, if not identical, with a similar figure by Dr. Pettigrew.

Professor Marey's figure occurs at page 200 of his new work (1874), that of Dr. Pettigrew's at page 338 of his memoir, "On the Physiology of Wings." (Trans. Roy. Soc. Edin., vol. xxvi., 1870).‖

A careful comparison of the figures in question will show that Professor Marey's figure is, or may be, a transcript of Dr. Pettigrew's. And this remark applies not only to the figure as a whole, but to all its details; first, to the horizontal direction of the figure-of-8 loops, made by the wing in space; secondly, to the reversal of the planes of the wing as the wing flies to and fro, *i.e.*, during a revolution; and thirdly, to the varying angles made by the surfaces of the wing with the horizon, when the wing is made to vibrate.

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* The following is the account given by Dr. Pettigrew: "The direction of the stroke varies slightly according to circumstances, but it will be quite proper to assume that the wing of the insect is made to vibrate in a more or less *horizontal* direction, and that of the bird or bat in a more or less *vertical* direction. By a slight alteration in the position of the body, or by a rotation of the wing in the direction of its length, the vertical direction of the stroke is converted into a horizontal direction, and *vice versa*.

"The facility with which the direction of the stroke is changed is greatest in insects; it is not uncommon to see them elevate themselves by a figure-of-8 *horizontal* screwing motion, and then suddenly changing the horizontal screwing into a more *vertical* one, to dart rapidly forward in a curved line."—Trans. Roy. Soc. Edin., vol. xxvi., p. 335.

† Physiologie—Détermination expérimentale du movement des ailes des insectes pendant le vol. Par M. E. J. Marey. Comptes Rendus, tom. lxvii., No. 26, December 28th, 1868, p. 1341.

‡ Professor Marey remarks—"We need only observe the flight of certain insects, the common fly for instance, and most of the other Diptera, to see that the plane in which the wings move *is not vertical*, but, on the contrary, *very nearly horizontal*."—(Animal Mechanism, 1874, p. 204).

‖ Figs. 5 and 6 more especially.

Surely this is more than a mere coincidence!

Then how strangely Professor Marey has blundered as to the direction of the stroke, when this is vertical. Thus he represents the wing (p. 195, fig. 82) as descending in a *downward* and *backward* direction, and as ascending in an *upward* and *backward* direction. Now this is simply a *physical* impossibility, and clearly shows that Professor Marey has failed to interpret the tracings obtained from the wing by his so-called graphic method.

The arrows in Professor Marey's figure-of-8 (*vide* figure 82), depicting the movements of the wing in space, should, in reality, be reversed. To get a continuous series of figure-of-8 loops, or of forward curves, characteristic of progressive flight, the wing must descend and ascend always *in a forward direction*, as described and figured by Dr. Pettigrew.* The tracings obtained by Professor Marey himself show this conclusively.

At page 201 of the work under consideration, Professor Marey describes his artificial wing as consisting of a *rigid main rib* in front and a flexible sail behind, from which it follows that he is not even now aware that a natural wing, and a properly constructed artificial one, are *flexible* and *elastic throughout*.

Professor Marey is wrong, when he states that the anterior margin of the wing of the insect *is rigid*. The following are his words:—

> "These experiments prove that the insect needs, for the due function of flight, a *rigid main rib* and a flexible membrane. If we cover the flexible part of the wing with a coating which hardens as it dries, *flight is prevented*. We hinder it also *by destroying the rigidity of the anterior nervure*."—P. 208.

Dr. Pettigrew, in his memoir "On the Physiology of Wings," expresses the facts in very few words:—

> "The wing of a flying creature . . . . *is not rigid*.† . . . That the anterior

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* According to this authority, "a natural wing, or a properly constructed artificial one, cannot be depressed either *vertically downwards*, or *downwards* and *backwards*. It *will* (the writer would say '*does*') of necessity descend *downwards* and *forwards in a curve*. This arises from its being flexible and elastic throughout, and in especial from its being carefully graduated as regards thickness, the tip being thinner and more elastic than the root, and the *posterior margin* than the *anterior margin*."

† This is again insisted upon in "Animal Locomotion," p. 240, where Dr. Pettigrew remarks, when speaking of the construction of an artificial wave wing on the insect type, "It should be *flexible and elastic throughout*."

margin of the wing should not be composed of a rigid rod may be demonstrated in a variety of ways. If a rigid rod be made to vibrate by the hand, the vibration is not smooth and continuous; on the contrary, it is irregular and jerky, and characterised by two *pauses*, the one occurring at the end of the *up stroke*, the other occurring at the end of the *down stroke*. The wing to be effective as an elevating and propelling organ should have no dead points, and should be characterised by a rapid winnowing or fanning motion. . . . . If a longitudinal section of bamboo cane has added to it tapering rods of whalebone which radiate in an outward direction, and this (framework)* be covered by a thin sheet of india-rubber (gutta-percha tissue), an artificial wing, resembling the natural one in all its essential points, is at once produced. . . . . If this wing be made to vibrate by its root, a series of longitudinal and transverse waves are at once formed, the one series running in the direction of the *length of the wing*, the other in the direction of its *breadth*. The wing further *twists* and *untwists* during the down and up strokes. . . . . This form of wing, which may be regarded as the realisation of the figure-of-8 theory of flight, elevates and propels both during the down and up strokes, and its working is accompanied with almost no slip. It seems literally to float upon the air.†

"*No wing that is rigid in the anterior margin can twist and untwist during its action, and produce the figure-of-*8 *curves generated by the living wing.* To produce the curves in question, the wing must be flexible, elastic, and capable of change of form in all its parts."‡

In one part of his new work, indeed (viz., at p. 198), Professor Marey seems to have largely profited by the observations and experiments of Dr. Pettigrew, as given above; for he states that, if rapid to-and-fro movements in a vertical plane be given to a "*flexible shaft*" (mark, the shaft is no longer described as *rigid*), to which he affixes a membrane similar to that found in the wings of insects—to use his own words—this *flexible shaft* will then represent the main rib of the wing; and we shall see this contrivance execute all the movements which the wing of the insect describes in space." "If," he says, "we illuminate the extremity of this artificial wing, we shall see that its point describes the figure 8 like a *real wing*; we shall observe also that *the plane of the wing changes twice during each revolution*, in the same manner as in the insect itself."—("Animal Mechanism," p. 198).‖

Professor Marey, it will be observed, claims for his artificial wing similar properties to those originally claimed by Dr. Pettigrew for his artificial wing. Thus Dr. Pettigrew states (*op. cit.*, pp. 421, 422), that if the anterior or thick margin of his artificial wave wing be directed upwards, and the wing made to vibrate, it will fly in *an upward direction* with an undulating motion; that if the anterior or thick margin of the wing be directed downwards, the wing will describe a waved track and fly *downwards;* and if the under surface of the wing makes no angle, or a very small angle with the horizon, it will dart forward in a series of curves *in a horizontal direction.*

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* The words in brackets are ours.

† Trans. Roy. Soc. Edin., vol. xxvi., 1870, pp. 408, 419, 420, and 422.

‡ Physiology of Wings. By J. Bell Pettigrew, M.D., F.R.S., p. 422. (Trans. Roy. Soc. Edin., vol. xxvi., 1870).

‖ The above remarks of Professor Marey are worth studying; for two reasons: first, because they are so confirmatory of all Dr. Pettigrew had written about the *flexibility* of the main nervure of an insect's wing; secondly, they contrast so strangely with the *rigid* main rib, at pp. 201 and 208 of Marey's work before cited."

Similarly, Prof. Marey says (p. 207) that if the anterior margins of the main ribs of his artificial insect be inclined upwards, the insect *rises vertically,* and that if the anterior margins of the main ribs be turned downwards *a descending vertical force is developed*; and that if the main ribs be turned upwards, and slightly forward, it developes the force *which sustains it in the air,* and directs its course in space.

We may point out many other parallel passages. Dr. Pettigrew states (*op. cit.*, p. 335)—

> "The direction of the stroke varies slightly, according to circumstances; but it will be quite proper to assume that the wing of the insect is made to vibrate in a more or less *horizontal direction,* and that of the bird and bat in a more or less *vertical direction.* By a slight alteration in the position of the body or by a rotation of the wing in the direction of its length, the vertical direction of the stroke is converted into a horizontal direction, and *vice versa.*
>
> "The facility with which the direction of the stroke is changed is greatest in insects; it is not uncommon to see them elevate themselves by a figure-of-8 *horizontal* screwing movement, and then suddenly changing the horizontal screwing into a more vertical one, to dart rapidly forward in a curved line."

Compare with the foregoing the following from Professor Marey's new work (p. 207):—

> "When an insect hovers over a flower, and we see it illuminated obliquely by the setting sun, we may satisfy ourselves that the plane of oscillation of its wings is *nearly horizontal.* This inclination must evidently be *modified* as soon as the insect wishes to dart off rapidly in any direction; but then the eye can scarcely follow it and detect the change of plane, the existence of which we are compelled to admit by the theory and the experiments already detailed."

When speaking of the wing of the bird, Dr. Pettigrew points out (Trans. Linn. Soc., vol. xxvi., p. 242) that—

> "The anterior or thick margin of the wing and the posterior or thin margin present different degrees of curvature, so that under certain conditions the two margins cross each other, and form *a true helix.* The anterior margin presents two well-marked curves, a corresponding number being found on the posterior margin.
>
> "These curves may, for the sake of clearness, be divided into *axillary* and *distal* curves; the former occurring towards the root of the wing, the latter towards the extremity.
>
> "The anterior, axillary, and distal curves completely reverse themselves during the acts of extension and flexion, and so of the posterior, axillary, and distal curves."

In like manner Prof. Marey, in his *first* chapter on the flight of birds (at p. 210), says that—

> "If we take a dead bird and spread out its wings . . . we see that, at different points in its length, the wing presents *very remarkable changes of plane.*

At the inner part, towards the body, *the wing inclines considerably both downwards and backwards*, while near its extremity it is *horizontal*, and *sometimes slightly turned up*, so that its under surface is directed somewhat backward."*

It is worthy of remark that the curves of the wing described and delineated by Dr. Pettigrew are reproduced by Prof. Marey (compare Figs. 68, 69, and 70 of Dr. Pettigrew's 1867 memoir, Linn. Soc. Trans., vol. xxvi., with the right wing, Fig. 89, p. 210, of Prof. Marey's new volume).

When speaking of the duration of the down and up strokes, Dr. Pettigrew observes (Trans. Linn. Soc., vol. xxvi., p. 261):—

> "In birds which glide or skim, it has appeared to me that the *wing is recovered much more quickly*, and *the downward stroke is delivered much more slowly*, than in ordinary flight; in fact, that the rapidity with which the wing acts in an upward and downward direction is, in some instances, more or less reversed; and this is what we would naturally expect if we recollect that in gliding the wings require to be, for the most part, in the expanded condition."

Prof. Marey writes in a similar strain. He states—

> "Experiment proves that the wing of the bird is raised more quickly than it descends."—(P. 212.) . . . "Contrary to the opinion entertained by some writers, *the duration of the depression of the wing is usually longer than that of its rise.* The inequality of these two periods is more distinctly seen in birds whose wings have a large surface and which beat slowly."—(P. 228.)

Weight, according to Dr. Pettigrew, contributes to horizontal flight. In illustration he states (Trans. Roy. Soc. Edin., vol. xxvi., pp. 355, 356)—

> "If two quill-feathers are fixed in an ordinary cork, and the apparatus allowed to drop from a height, the cork does not fall vertically downwards, but *downwards and forwards in a curve.* When artificial wings, constructed on the principle of natural ones, are allowed to drop from a height, they *describe double curves* in falling, the roots of the wing reaching the ground first, which proves the greater buoying power of the tips of the wings. Weight, when acting upon wings, must be regarded as an independent moving power." . . . "The *wings of the bird form a natural parachute*, from which the body depends both during the down and up strokes."—(P. 371.)

Prof. Marey performs similar experiments, and arrives at similar conclusions. Thus he explains (p. 217) that if a sheet of paper folded in the middle, with a wire loaded at one end and fixed in the bent portion, be allowed to fall, the apparatus will not descend vertically, but will follow an *oblique trajectory*; and that if the corners of the paper be bent, and the concavity directed downwards, the apparatus will in falling describe a *double curve*.

> "The wings are attached exactly at the highest part of the thorax, and consequently, when the outstretched wings act upon the air as a fulcrum, all the weight of the body is placed below this surface of suspension. Thus the

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*** It is evident, from the succeeding paragraph to above quotation from "Animal Mechanism," that Prof. Marey had read Dr. Pettigrew's observations to which we have just referred.**

heaviest part is placed as low as possible beneath the point of suspension. The bird, as it descends with its wings outspread, will thus present its ventral region downwards, without its being necessary to make an effort to keep its equilibrium; it will take this position passively, *like a parachute set free in space*, or like the *shuttlecock when it falls upon the battledore*."—("Animal Mechanism," p. 216.)

Dr. Pettigrew likens the wing of the bird to a boy's kite (Proc. Roy. Inst. of Great Britain, March 22, 1867)—

> "The wing of the bird *acts after the manner of a boy's kite*, the only difference being that the kite is pulled forwards upon the wind *by the string and the hand*, whereas in the bird the wing is pushed forwards on the wind by *the weight of the body* and the life residing in the pinion itself."

Similar in substance is the subjoined passage from Prof. Marey (p. 220):—

> "In the last two forms, the wing, directed more or less obliquely, derives its point of resistance from the air, *like the child's plaything called a kite*, but with this difference—that the velocity is given to the kite by *the tractile force exerted on the string* when the air is calm, while the bird when it hovers utilises the speed which it has already acquired either by *its oblique fall* or by the previous flapping of its wings."

Dr. Pettigrew attaches great importance to the activity of the wing and its small size. Thus he remarks (Trans. Roy. Soc. Edin., vol. xxvi., p. 408)—

> "*The surface exposed by a natural wing*, when compared with the great weight it is capable of elevating, *is remarkably small*. This is accounted for by the length and *great range of motion* of natural wings, the latter enabling the wings to convert large tracts of air into supporting areas. It is also accounted for by the *multiplicity of the movements of natural wings*, these enabling the pinions to create and rise upon currents of their own forming, and to select and utilise existing currents." . . . "The *problem of flight would seem to resolve itself into one of weight, power, velocity, and small surfaces*, as against comparative levity, debility, diminished speed, and extensive surfaces." —(P. 386.)

Analogous in many respects to the foregoing is the following from Prof. Marey (p. 222):—

> "The part played by the wing in flight is not merely passive, for a sail or a parachute ought always to have a surface in proportion to the weight which it has to support; but, on the contrary, when considered in its proper point of view, *as an organ which strikes the air*, the wing of the bird ought, as we shall see, to present a surface *relatively less in birds of a large size and of great weight*."

Again:—

> "Animals of large size and great weight sustain themselves in the air *with a much less proportionate surface of wing* than those of smaller size."—(P. 222.)

Dr. Pettigrew dwells upon the relative speed attained by the different parts of the wing (Trans. Roy. Soc. Edin., vol. xxvi., pp. 399—442). He says the wing as a rule is long and narrow.

> "As a consequence *a comparatively slow and very limited movement at the root confers great range and immense speed at the tip*, the speed of each portion

of the wing increasing as the root of the wing is receded from." . . . "The small humming bird, in order to keep itself stationary before a flower, requires to oscillate its tiny wings with great rapidity, whereas the large humming bird can attain the same object by flapping its large wings with a very slow and powerful movement." . . . "In the larger birds the movements are slower in proportion to the size, and more especially in proportion to the length, of the wing. This leads me to conclude that *very large wings may be driven with a comparatively slow motion*."

Professor Marey illustrates the same points as under (p. 224):—

> "It is not immaterial whether the surface which strikes the air has its maximum near the *body* or near the *extremity* ; *these two points have very different velocities*. For an equal extent of surface the resistance will be greater at the point of the wing than at its base."

Again (p. 226):—

> "It can be proved that, if the strokes of the wing were as frequent in large as in small birds, each stroke would have a velocity whose value would increase with the size of the bird; and as the resistance of the air increases, for each element of the surface of the wing, according to the square of the velocity of that organ, *a considerable advantage would result to the bird of large size, as to the work produced upon the air*."

Dr. Pettigrew shows that the vigour with which the wing is propelled varies according as the bird is rising, falling, or progressing in a horizontal direction (Trans. Linn. Soc., vol. xxvi., pp. 227, 260, 261). He observes:—

> "All birds which do not, like the swallow and humming birds, drop from a height, raise themselves at first by a vigorous leap, in which they incline their bodies in an upward direction. *By a few sweeping strokes*, delivered downwards and forwards, in which the wings are nearly made to meet above and below the body, they lever themselves upwards and forwards, and in a surprisingly short space of time acquire that degree of momentum which greatly assists them in their future career." . . . "The forward movement of the wing during the down or effective stroke is particularly evident in birds when rising, the wing on such occasions being urged with *unusual vigour*."—(P. 227, *op. cit.*)
>
> "When the bird has elevated itself to the desired height, *the length of the downward stroke is generally curtailed*, the mere extension and flexion of the wing, assisted by the weight of the body, in some cases sufficing for the ordinary purposes of flight. This is especially the case if the bird is advancing against a slight breeze."—(Pp. 260 and 261.) "If birds wish to descend, they may reverse the direction of the inclined plane, and plunge head foremost with extended wings; or they may flex the wings, and so accelerate their pace; or they may raise their wings, and drop parachute fashion; or they may even fly in a downward direction—*a few sudden strokes*, a more or less abrupt curve, and a certain degree of horizontal movement, being in either case necessary to break the fall previous to alighting."—(P. 262, *op. cit.*)

Prof. Marey, as the annexed passages show, also adverts to the relative frequency and force with which the wing is urged in ascending, descending, and horizontal flight, though less fully than Dr. Pettigrew does:—

> "The frequency of the strokes of the wing varies also according as the bird is first starting in full flight or at the end of its flight."—(P. 228, "Animal Mechanism.")

Again :—

"Confining the question within these limits, experiment shows that the strokes of the bird's wing *differ in amplitude and in frequency* from one moment to another as they fly. When they first start *the strokes are rather fewer, but much more energetic ;* they reach, after two or three strokes of the wing, *a rhythm almost regular, which they lose again when they are about to settle.*" —(P. 234, *op. cit.*)

Dr. Pettigrew lays especial emphasis on the elliptical movements made by the wing of the bird (Trans. Linn. Soc., vol. xxvi.) Thus he remarks :—

"During extension the elbow and bones of the fore-arm, particularly their distal extremities, describe an *upward curve.* During flexion the elbow and bones referred to describe another but *opposite curve.* The movements described by the elbow-joint during extension and flexion may consequently be represented by an *ellipse or ovoid.*"—(P. 248, *op. cit.*, Diagrams 8 to 13 more especially.)

Prof. Marey follows Dr. Pettigrew in the matter of these elliptical movements. He says :—

"During the whole of the bird's flight the registering lever described *a kind of ellipse.*" . . . "All our experiments have shown that birds of different species describe with their wings *an elliptical trajectory.*"—(P. 242.)

Again :—

"In fact, the bone of the wing in each describes a kind of *irregular ellipse,* with its greater axis inclined *downwards and forwards.*"

Dr. Pettigrew represents the wing of the bird as oscillating on two separate axes,—the one running parallel with the bird, the other at right angles to it,—and adds (in his 1867 memoir to the Linnean Society, p. 243)—

"The wing may be said to agitate the air in two principal directions, viz., from *within outward or the reverse,* and from *behind forward or the reverse,* the agitation in question producing two powerful pulsations—a longitudinal and a lateral."

Prof. Marey gives, more briefly but yet in very similar terms, the same statement. He says (p. 247)—

"That a bird passes through a *double oscillatory movement,* in a vertical plane, for each revolution of its wings."

Dr. Pettigrew describes and delineates the wing of the bird as advancing in a curved line, both when it rises and falls. He observes (Trans. Linn. Soc., vol. xxvi., pp. 214 and 233)—

"In the water the wing strikes *downwards and backwards* (and acts as an auxiliary of the foot), whereas in the air it strikes *downwards and forwards.* . . . To counteract the tendency of the bird in motion to fall in a downward and forward direction, the stroke is delivered in the direction in which falling would naturally occur,—the kite-like action of the wing, and the rapidity with which it is moved, causing the mass of the bird to pursue a more or less horizontal direction. I offer this explanation of the action of the wing in and out of the water after repeated and careful observation in tame and wild birds, and, as I am aware, in opposition to all previous writers on the subject."

Prof. Marey again corroborates Dr. Pettigrew's original observations, as the subjoined extract from "Animal Mechanism" will show (p. 254) :—

> "The inspection of the curve shows us also that the pigeon's wing was carried more especially in the direction of the upper parts, similar to the point A; in other terms, that *the forward predominated over the backward movement.*"

Dr. Pettigrew describes and figures the body of the bird in flight as alternately rising and falling in forward curves, the curves described by the body being the opposite of those described by the wing, those movements being due to a kite-like action of the wings (Trans. Roy. Soc. Edin., vol. xxvi., pp. 343, 344). Thus Dr. Pettigrew remarks :—

> "It is a condition of natural wings, and of artificial wings constructed on the principle of living wings, that, when forcibly elevated or depressed, even in a strictly vertical direction, they inevitably dart forward. In both cases the wing describes *a waved track*, which clearly shows that the wing strikes downwards and forwards during the down stroke, and upwards and forwards during the up stroke. The wing, in fact, is always advancing, *its under surface attacking the air like a boy's kite.*" . . . . "As the body of the insect, bat, and bird, falls forward in a curve when the wing ascends, and is elevated in a curve when the wing descends, it follows that *the trunk of the animal is urged along a waved line.* I have distinctly seen the alternate rise and fall of the body and wing, when watching the flight of the gull from the stern of a steamboat."

Professor Marey writes in a very similar strain. He asks (pp. 264, 265) :—

> "But do we find that the bird, when suspended in the air, keeps at a constant level, or *does it pass through oscillations in the vertical plane?* Do we not experience, by the intermittent effect of the flapping of its wings, *rising and falling motions*, of which the eye can detect neither the frequency nor extent? Again, does not the bird advance in its onward course, with variable rapidity? Shall we not find in the action of its wings a *series of impulses*, which give to its advancing course a *jerking motion?* These queries can be answered experimentally." . . . . "To explain the ascent of the bird during the time of the elevation of the wing, it seems indispensable to refer to *the effect of the child's kite*, to which we have before alluded. The bird, having acquired a certain velocity, presents its wings to the air *as inclined planes.*" . . . . "Thus, by registering at the same time the two orders of oscillation in the flight of a buzzard, we find that the phase of *depression of the wing* produces at the same time the *elevation of the bird* and the acceleration of its horizontal swiftness."—(P. 269). . . "A part of this resistance, viz., that which is applied to *the lower surface of the wing*, is utilised to sustain the bird by the kind of action which we have compared to that of *a child's kite*. It appears that this action is of primary importance in the flight of the bird."—(P. 275). . . "In the bird, one of the phases of the movement of the wing is, to a certain extent, passive; that is to say, it receives the pressure of the air *on its lower surface, when the bird is projected rapidly forward by its acquired velocity.* Under these conditions, the whole bird, being carried forward in space, all the parts of the wing are moved with the same rapidity, to take advantage of the action of the air, *which presses on them as on a kite.*"—(P. 276).

Dr. Pettigrew explains that the wing is a screw structurally and functionally; that it revolves on two axes (the one running in the direction of the length of the wing, the other in the direction of its breadth), and that the action of the wing resembles the action of an oar in sculling. (Trans. Linn. Soc., vol. xxvi., pp. 206, 229, 231, and 266; Proc. Roy. Inst. of Gt. Britain, March 22, 1867). Thus, he states that:—

> "In the fish, the lower half of the body and the broadly expanded tail are applied to the water *very much as an oar is in sculling*." . . . . The fish may be said to drill the water in two directions, viz., from behind forward, by a *twisting or screwing* of the body on its long axis, and from side to side by causing its anterior and posterior portions to assume opposite curves. The pectoral and other fins are also thrown into curves in action, the movement, as in the body itself, travelling *in spiral waves*; and it is worthy of remark that the wing of the insect, bat, and bird obeys similar impulses.
>
> "The fins *are rotated or twisted*, and their free margins lashed about by spiral movements, which closely resemble those by which the wings of insects are propelled. . . . . That the wing *twists upon itself structurally*, not only in the insect, but also in the bat and bird—anyone may readily satisfy himself by a careful examination; and that it *twists upon itself during its action* I have had the most convincing and repeated proofs." "The wing of the bird acts *as a twisted inclined plane*. In this respect it intimately agrees with the wing of both the insect and bat. . . . . The twisting in question is most marked in the posterior, or thin margin of the wing, the anterior or thicker margin performing more *the part* of *an axis*. As the result of this arrangement, the anterior or thick margin cuts into the air quietly, and as it were by stealth, the posterior one producing on all occasions *a violent commotion*, especially perceptible if a flame be exposed behind the insect."

Professor Marey goes over the same ground in much the same way. Thus, at pages 107, 109, 198, 208, 210, 211, 259, and 261, he states:—

> "The oar is found in many insects which move on the surface of the water. A contrivance is employed by other animals, which resembles the action of an oar used at the stern of a boat in the process called *sculling*. To the latter motive power may be referred all those movements in which *an inclined plane* is displaced in the liquid, and finds in the resistance of the water which it presses obliquely two component forces, of which one furnishes a movement of propulsion." . . . . "When a fish strikes the water with his tail, in order to drive himself forward, he executes a double work, a part tends to drive behind him a certain mass of fluid with a certain velocity, and the other to drive the animal forward, in spite of the resistance of the surrounding water. . . . . *Aërial Locomotion.*—This mechanism is still the same; the motion of *an inclined plane*, which causes motion through the air, the wing in fact, in the insect as well as in the bird, strikes the air *in an oblique manner*, repels it in a certain direction, and gives the body a motion directly opposite." . . . . . "Each stroke of the wing *acts upon the air obliquely*, and neutralises its resistance, so that a horizontal force results, which impels the insect forward. An effect is produced analogous with that which takes place when an oar is used in the stern of a boat in the *action of sculling*." "Most of the propellers which act in water overcome the resistance of the fluid by the action of *an inclined plane*. The tail of the fish produces a propulsion of this kind. *Even the screw may be considered as an inclined plane*, whose movement is continuous, and always in the same direction. . . . . We see the main rib (anterior margin) of the wing remain *sensibly immovable, and around it turns the membranous portion* (posterior margin). . . . . "If this motion as on a pivot did not exist, the wing would cut the air with its edge, and would be utterly incapable of producing flight." . . . . The wing presents *very remarkable changes of plane* at the *inner part towards* the body; the wing

> *inclines considerably*, *both downwards* and *backwards*; while near its extremity it is *horizontal and somewhat slighty turned up*." . . . . We admit that the wing *revolves on an axis*." . . . . "It was necessary therefore, for the lever, while fixed to the feathers of the bird, to glide freely on the rod in the direction of its length; and yet that it should cause it to receive, under the *form of torsion*, all the changes of inclination that are transmitted to it by the wings of the bird." . . . . "For this purpose we must return *to the dotted figure* 8, which is the impression of the *torsions of the wing* of the different instants."

Dr. Pettigrew points out that the wing acts as a true kite, both during the down and up strokes. He remarks—(Trans. Roy. Soc. Edin., vol. xxvi., p. 343):—

> "If, as I have endeavoured to explain, the wing, even when elevated and depressed in a strictly vertical direction, inevitably and invariably darts forward, it follows, as a consequence, that the wing flies forwards as a true kite, both during the down and up strokes, and that its *under concave, or biting surface*, in virtue of the forward travel communicated to it by the body in motion, is *closely applied to the air, both during its ascent and descent*; a fact hitherto overlooked, but one of considerable importance, as showing how the wing furnishes a persistent buoyancy alike when it rises and falls. The angle made by the wing of the bat and bird with the horizon is constantly varying, as in the insect wing."

Professor Marey, in his earlier writings ("Revue des Cours Scientifiques de la France et de l'Etranger," Mars, 1869) describes the wing as making *a backward angle of* 45° *with the horizon during its descent, and a forward angle of* 45° *during its ascent.*

This view was shown by Dr. Pettigrew to be untenable, and we find it greatly modified in Professor Marey's later work. Thus, at fig. 111, p. 263, where the angles of inclination made by the wing during its rise and fall are given, the under surface of the wing is represented as *forming a kite*, during quite three-fourths of one entire revolution of the wing.

At no point is the wing represented as making, during its descent, *a backward angle of* 45°. Nor is this all. At p. 274, Professor Marey states that—

> "In free flight the axis of the bird is horizontal, or rather *turned somewhat upward*. Restored to this proper position, a fresh direction would be given to each of the positions of the wing. Then, probably, we should see that the wing always *presents its lower surface to the air*, as the only one which can find in it a point of resistance."

In this modified statement, as may readily be perceived, we have simply a repetition of Dr. Pettigrew's view, viz., "*that the under surface of the wing acts as a kite, both when the wing rises and falls.*"

We might greatly multiply these parallel passages in proof of our original assertion, that in all Professor Marey's writings and experiments in flight, *Dr. Pettigrew's original discoveries and experiments may be traced*, and we may fairly emphasise our other statement, "*that Professor Marey has done scant justice to Dr. Pettigrew.*

This is the more evident, as we are given to understand that Dr. Pettigrew's original memoirs and papers were duly transmitted to Professor Marey immediately after their publication.

From the foregoing, it will be evident that Professor Marey has added comparatively little to the science of Aërostation. He has, for the most part, simply confirmed by experimental methods, in which he is an adept, Dr. Pettigrew's original observations and experiments, published nearly two years before his own experiments were undertaken.

Professor Marey is certainly not entitled to say, as he does at p. 187, that "*notwithstanding this apparent agreement, our theory and that of Dr. Pettigrew differ materially from each other.*"

Still less is he entitled, virtually, to appropriate Dr. Pettigrew's descriptions and figures, without full and fair acknowledgment. Least of all is he entitled to modify and misrepresent those descriptions and figures.

Such practices sap the foundation of science.

NOTICES OF BOOKS.

The Aerial World: A Popular Account of the Phenomena and Life of the Atmosphere. By G. HARTWIG, M. & P.D. London: Longmans, Green, and Co. 1874.

IN this work we find a complete chemical and physical history of the atmosphere; the facts of all the sciences are brought forward to elucidate the subject: the physical properties of the atmosphere, sound and echoes, light in connection with the colours of the sky, heat in connection with the temperature of the air, winds, &c., electricity in connection with thunderstorms, St. Elmo's fires, and electrical meteors; of each and all of these we have a full description.

The first chapter treats of the weight and pressure of the air. In regard to the effect of rarefied air on the system, the author remarks that it is scarcely likely that any one will in future be foolhardy enough to endeavour to ascend above six miles, seeing how nearly Messrs. Coxwell and Glaisher lost their lives at the time of their famous balloon ascent. It has, however been suggested, that people who are in the habit of living in rarefied air—as the inhabitants of Quito and Potosi—might be able to reach a greater altitude than those who live at or near the level of the sea; and this experiment, which has never been tried, is certainly worth trying. In the course of a few generations of life, at high altitude, it is by no means improbable that the system might adapt itself to the surrounding circumstances, and that an extension of this rarefaction might be borne without difficulty.

The second chapter treats of the chemical composition of the atmosphere, and the various gases which compose it are described. In speaking of ozone, the author quotes the "Odyssey" to show that the peculiar sulphurous smell which accompanies manifestations of electricity has been known from the time of Homer. He omits, however, that extremely interesting philological fact, that the Greek for sulphur, θειον, was derived from this cause. Zeus manifested himself by thunderbolts; the sulphurous smell accompanied such manifestations; hence, clearly sulphur, in some way or other, appertains to the gods; hence it is assumed that it was called θειον (θειος); and we have the echo of the old Greek word in some of our more complex sulphur acids—trithionic, tetrathionic, pentathionic. In the third chapter we are introduced to the science of Acoustics, and come again to the mighty Zeus, in whose forest of Dodona the rustling of the oak leaves was believed to be the voice of the god himself. It is thus that the author blends mythology and history with the more sober facts of science. The fifth chapter contains some interesting remarks concerning the colours of the sky, which question has been debated any time since that of Leonardo da Vinci. He and many subsequent writers attributed the celestial blue to a mixture of the light reflected from terrestrial substances with the darkness of space. Goethe, in his "Farbenlehre," revives the same idea: he points out that, when we look through a turbid medium which is illuminated, at a black background, the medium appears to be blue; while light itself, viewed through such a medium, is yellow or red. He points out also the blueness of distant mountains, and of smoke seen in particular lights. The author then alludes to Tyndall's experiments, and with him concludes that the blue colour of the sky is due to the presence of extremely minute particles of water. On pp. 80-81, we find a few inaccuracies of printing; thus temperatures are written like degrees of arc, 17° 6′ F. 50° 4′, and (p. 81) the *minus* is omitted in stating extremes of cold:—53°, 58°. The table pp. 82-83, is also printed rather carelessly in the case of the *Latitude* column. Among the low European temperatures we find −5° F. near London, −10·5° in Paris, −22° in Hamburg, −37° in St. Petersburg, −44° in Moscow, and −58° at Enontekis in Lapland. The records of several winters in Europe are most surprising; it is said that in 860 and 1234 the Adriatic was frozen over, and goods were transported from Venice to the opposite Dalmatian coast, over the ice. In 1364 all the rivers of France were frozen over, and the ice on the Rhone was 15 feet thick. The winter of 1788-89 appears to have been one of the severest on record. The Rhone was frozen over at Lyons, and the port of Ostend was closed by ice; and the Thames was frozen as far as Gravesend. As to European extremes of heat, we learn that in 1793 the thermometer in London registered 89° F., and in Paris, 101° F.; in each instance in the middle of July. In 1852, the thermometer in London rose to 95° F. in the shade on July 12th.

In the seventh chapter, "On Winds," the author devotes several paragraphs to an account of the *Tower of the Winds* in Athens, using always the past tense, "Boreas *was* represented," other winds "*were* represented." But there is no need for this. We may use the present tense for many long years to come; the Tower of the Winds is quite perfect, and is of comparatively late erection, that is to say, 100 B.C. The description of the monsoons, and the causes which produce them, is as complicated and difficult to understand as it is in every book in which we have seen them mentioned and described. An interesting account of a thunderstorm seen from a balloon, and a "side view of a storm," will be found in the fourteenth chapter. We notice in the woodcuts the usual error in regard to lightning, which is made pointed, whereas, as Faraday pointed out, if we could see the end of the flash at all, it would be at least as thick as the rest of the flash. Remarkable examples are recorded of the power of lightning. Deaths from lightning were most numerous in 1870, 202 deaths were recorded throughout the whole country: the deaths from sunstroke are twice as numerous. The instantaneous appearance of *rigor mortis* in the case of persons killed by lightning appears to be well authenticated. A curious slip of the pen may be noticed on p. 289, where the author, in mentioning the fact that the Emperor Augustus used to retire to a cave on the approach of a thunderstorm, puts him and his adjective in the accusative case:—"divu*m* Augustu*m*." The Romans considered seal-skins a preventive to lightning, and the emperor Tiberius caused himself, on the approach of a storm, to be crowned with laurel, believing that lightning never struck the plant sacred to Apollo.

An interesting and novel chapter is devoted to "The Primeval Atmosphere." In this the author points out that during the Silurian epoch—the earliest of all—the atmosphere must have been as transparent as it is now; he questions whether the air during the luxuriant carboniferous epoch can have had much more carbonic acid in it than it has now, for Liebig has shown, that if all the carbon contained in all living vegetation and in the coal measures was in the atmosphere again in the form of carbonic acid, the quantity of that gas in the air would scarcely be doubled, and would not be unfit for the respiration of the higher animals. While the composition of the air may not have undergone much change the temperature certainly has, for coal has been discovered in the Arctic regions, and in it plants that could only grow in a temperate climate. Again, during the Miocene Period, the temperature of Europe must have been at least sub-tropical; the flora somewhat resembles that of South Carolina, while apes abounded in the Pyrenees and in Greece. The latter chapters of the work are devoted to flying and ballooning.

The work, from beginning to end, is pleasantly written, and is very readable; it is crowded with facts derived from every available source, and however dry they may be in the abstract, they are clothed here in popular language, and their true interest is shown. The work is well printed, and beautifully illustrated by chromoxylographic plates, and by a good map and woodcuts. It belongs to the "Guillemin" and "Flammarion" class. Such books do much to induce a love for science and for Nature, and we cordially recommend this work to the notice of all our readers.

Science Primers. Astronomy. By J. Norman Lockyer, F.R.S. Illustrated. Macmillan and Co. 1874.

THIS little work forms the sixth of the series of Science Primers, edited by Professors Huxley, Roscoe, and Balfour Stewart. Although modestly called a Primer, it contains an immense deal of information, and discusses sun-spots, nebulæ, and multiple stars, while it further makes clear to us the meaning of such terms as "polar distance" and "right ascension." The language throughout is of the clearest; the book is well illustrated, and the author has throughout described simple experiments to illustrate his meaning. By using a moderator lamp to represent the sun, an orange with a knitting-needle stuck through it to represent the earth, a tub of water with floating balls, a small suspended ball, a round table with a ball placed in the centre, and a few simple contrivances of this sort, a number of important phenomena are made clear to the learner. The following is an example of the author's style:—"Thus, besides the planets, there are other members of the system, namely comets and falling stars, which will be mentioned again more fully hereafter. All these bodies form a sort of family having the sun for their head, and on Plate II. will be seen a view of this system as it would appear when looked at from above; but it is impossible thus to give an idea of the true scale of the system. In order to do this, take a globe a little over two feet in diameter to represent the sun: Mercury would now be proportionately represented by a grain of mustard seed, revolving in a circle 164 feet in diameter; Venus, a pea, in a circle of 284 feet in diameter; the earth also a pea, at a distance of 430 feet; Mars, a rather large pin's head, in a circle of 654 feet; the smaller planets by grains of sand, in orbits of from 1000 to 1200 feet; Jupiter, a moderate-sized orange, in a circle nearly half-a-mile across; Saturn, a small orange, in a circle of four-fifths of a mile; Uranus, a full-sized cherry or small plum, upon the circumference of a circle more than a mile and a half; and Neptune a good sized plum, in a circle about two miles and a half in diameter."

Introduction to Experimental Physics, Theoretical and Practical; including Directions for Constructing Physical Apparatus, and for Making Experiments. By Adolph F. Weinhold, Professor in the Royal Technical School at Chemnitz. Translated by Benjamin Loewy, F.R.A.S. Preface by G. C. Foster, F.R.S. London: Longmans, Green, and Co. 1875.

In the Preface to this work, Professor Foster says:—"I am convinced that the true way to make the somewhat abstract notions necessarily encountered at the outset of the study of physics intelligible to beginners, is not to emphasize the abstractions, but to provide the learner with the clearest possible ideas of the concrete facts from which the abstractions are derived. In any sound system of teaching, particulars must come before generalities; for, unless a student has clear conceptions of individual phenomena, it is impossible for him to understand their mutual relations or the general conclusions that are based upon them." To provide an account of these concrete physical facts, and to show how they may best be wrested from Nature, has been the object of Professor Weinhold in the book before us. He takes us through a course of elementary physics, and shows us, not only how to make the experiments, but also how to make the necessary apparatus. If anything, his descriptions appear sometimes to be too tediously minute, but for the beginner they are certainly not so. Here, for example, we have directions for boring a hole in glass—an operation which is frequently necessary in physical experiments:—"The hole is bored with a round file, of which the point is broken off, so that a round surface of from 2 to 3 m.m. in diameter is obtained. The edge of this round surface is used for drilling, the file being held with the right hand in a slanting direction, and the thumbs being kept quite close to the broken end of the file, in order to increase the pressure, and also to prevent the file from slipping through the finished hole and breaking the glass. File and glass must, during the operation, be frequently welted with water, or, better still, with oil of turpentine. After piercing the glass completely by the point, the whole is enlarged by slowly turning the file from right to left in the manner in which a screw is drawn, repeatedly moistening the orifice and the file." The author truly says it is advisable to practise this operation first on pieces of broken glass; for, if ever patience be wanted, it is when one is engaged in boring a hole in glass, or endeavouring to render the double needle of a galvanometer astatic. Let us glance at the author's treatment of any individual science; for example, *heat*. Omitting all theoretical matter, the author plunges at once into the subject; tells us that there is a particular sensation depending upon change of temperature, and that to the cause of this sensation is given the name Heat. Of the various effects of heat upon matter, expansion comes first, and this is illustrated by various means in the case of solids, liquids, and gases; and various experimental hints and cautions are given. An account of the thermometer follows, and rules for the conversion of degrees, and for determining the fixed points; then various effects of expansion, Prince Rupert's drops and Bologna flasks; anomalous expansion of water, absolute and apparent expansion, and the expansion of gases. A section is then devoted to melting and freezing, followed by evaporation and ebullition; radiation and conduction; specific and latent heat, and sources of heat. There is thus nothing new in the arrangement of subject matter, or indeed in the treatment of it, save that each experiment is minutely described, the necessary precautions and the conditions of success. The young student of physics will find this work a most useful companion.

Elements of Magnetism and Electricity. By JOHN ANGELL, Senior Science Master, Manchester Grammar School, with 120 illustrations. London and Glasgow: William Collins, Sons, and Co. 1875.

THIS very cheap and comprehensive little manual belongs to the capital series of elementary works on science which Messrs. Collins are now issuing. It is almost entirely devoted to Magnetism and Frictional Electricity, one chapter only at the end of the book being devoted to Voltaic Electricity. The figures are plain, but quite sufficient for their purpose; we may particularly call attention to the representations of magnetic curves (Figs. 19—23), and to the figures showing electrical distribution on various surfaces (Fig. 89). Hints are frequently given for the construction of cheap apparatus, but these cannot always be relied upon; for example, we are told how "an efficient electrical machine," capable of performing all the ordinary experiments, may be constructed out of a common black wine-bottle for a "few pence;" but all who have tried to make an electrical machine know how very difficult it is to get anything like an efficient machine at a cost of much time and many shillings. There are a thousand difficulties in the way: to mount the cylinder—to make it rotate in a horizontal plane—to manipulate the even pressure of the rubber—are among them. Again, some of the explanations are insufficient; for example, in describing the electrical whirl, the author says:—"On working the machine, a powerful wind, the electrical aura, will be produced, which, reacting against the points, will drive the wheel round with great velocity." But he does not explain why the wind is produced. It is a general fault in elementary works, in which as much matter as possible is introduced at the expense of clearness, to describe experiments without fully explaining them. But the book is distinctly good, and a marvel of cheapness, and will be found most useful as a commencing text-book.

Elements of Animal Physiology, chiefly Human, with Hints on Practical Work, Dissection, &c. By JOHN ANGELL, Senior Science Master, Manchester Grammar School. London and Glasgow; William Collins and Sons. 1875.

THIS is a shilling book containing 92 capital figures, and more than 500 separate paragraphs of text. It is a marvel of cheapness, and although we must of course regard it as, to a great extent, a compilation, it contains a great amount of sound information and useful knowledge. It is divided into twenty chapters: the first two treat of the general structure and functions of the human body; here we notice a complete comparison of the actions of a living body with those of a steam engine, in which we learn that the total *internal* mechanical work of a living man is no less than 715,000 foot-pounds per day, which Helmholtz calculates is equal to the total *external* work of a hard-working man. It has also been calculated that 2¼ pounds of good coal, if its force could all be utilised, ought to produce in a steam-engine 2,145,000 foot-pounds of mechanical work, or about three times the external work of a man. The third chapter treats of the skeleton; the fourth and fifth of chemical and histological preliminaries; in the latter, very good drawings of voluntary muscular fibre and of the structure of nerve fibre. The sixth and seventh chapters discuss the blood; the eighth and ninth, respiration and animal heat; the tenth and eleventh, digestion (beautiful drawing of a vertical section of the coats of the stomach); an interesting chapter on animal mechanics, and concluding chapters on the voice, the ear, the eye, and the brain. The author appears to be quite *au courant* with the most recent results of physiology, and we heartily recommend his little treatise to those who desire to commence the study.

The Methods of Ethics. By HENRY SIDGWICK, M.A., Lecturer and late Fellow of Trinity College, Cambridge. London: Macmillan and Co. 1874.

DEFINING ethics as "the Science of Conduct," the author shows that it may conveniently include the related studies of politics and jurisprudence. These sciences alike attempt to determine the nature of the ideal, not the real; to demonstrate what ought to exist, not what does exist. The rational ends are divided into two only, *i.e.*, Perfection and Happiness, and the methods of Ethics are reduced in the main to three—Egoism, Intuitionism, and Utilitarianism. The end of Egoism is defined as "the sum of pleasures, valued in proportion to their pleasantness;" the fundamental assumption of Intuitionism is "that we have the power of seeing clearly what actions are in themselves right and reasonable;" while by Utilitarianism is meant the theory first distinctly enunciated by Bentham "that the conduct which, under any given circumstance, is externally or objectively right, is that which will produce the greatest amount of happiness to all whose interests are affected." The work, admirable as it is, possesses little interest for the man of science, as ethical modes of thought are altogether dissimilar to the usual modes of scientific thought; we may briefly glance, however, at the chapter on "Philosophical Intuitionism." A philosopher is allowed at the outset some amount of divergence from common sense, and he is expected to altogether transcend common sense in his premises, and to sink his plummet into profound and infinite depths. But such men have often, after a life of study, presented to the world a host of arguments and conclusions which move in a narrow circle, or a host of propositions swollen out with tautological excesses, and like a windbag ready to burst on the application of the least pressure. And here we venture to think the philosopher of science has the advantage of the philosopher of morals; for the former can scarcely argue in a circle when he is confronted at every step by facts wrought out of nature by legitimate experiment and exact induction.

The Transit of Venus: its Meaning and Use. By T. H. Budd, F.R.A.S. London: Longmans, Green, and Co. 1875.

In this clever and concise little pamphlet the author tells us, in plain untechnical language, all that a general reader interested in science, but not specially an astronomer, can require to know. We will briefly condense the main facts of the case. The transit of Venus means, we all know, the passage of Venus across the face of the sun; and since the moon eclipses the sun under like circumstances, and Venus has thrice the diameter of the moon, we may reasonably enquire why the sun is not eclipsed during the transit of Venus. But the fact that Venus is far more distant from us than the moon converts what would be an eclipse into a transit. The transit occurs twice in 116 years, with an interval of 8 years between two successive transits in that period, *e.g.*, in 1761 and 1769, and again in 1874 and 1882. The long interval is due to the fact that the plane of the orbit of Venus is inclined to the plane of the earth's orbit round the sun. The exact observation of the transit of Venus enables astronomers to ascertain our distance from the sun; and as it is believed that an error of three millions of miles exists in our present estimate of that distance, the necessity of a new determination is obvious. There must always be some error, but repeated observations will reduce it to a minimum. The Author points out and illustrates by a diagram how, by means of a theodolite and a simple trigonometrical method, it is possible to measure an inaccessible object, and how this method may be applied to the sun when a suitable base line has been obtained. The greatest possible distance between two observers on the earth's surface—that is, the greatest possible base line on one and the same meridian—is something less than 7000 miles, because the polar regions are inaccessible. The observation of the different positions of Venus when crossing the sun's face, by two distant observers, enables us to ascertain the diameter of the sun, and by comparing his apparent diameter with his real diameter we can ascertain his distance from us in the same way that we can determine the distance of a church steeple if the height be known.

The Origin of Civilisation and the Primitive Condition of Man: Mental and Social Condition of Savages. By Sir JOHN LUBBOCK, Bart., M.P., F.R.S. Third Edition, with numerous Additions. London: Longmans, Green, and Co. 1875.

THIS volume, which has already reached a third edition, is founded on the subject-matter of a course of lectures delivered at the Royal Institution in 1868. It embodies the result of ten years of study, and constitutes a considerable addition to our study of Anthropology. The great bulk of the work treats of the religion of savages, and this subject is treated very ably in some concluding remarks:—"Thus we see that as men rise in civilisation their religion rises with them. The Australians dimly imagine a being,—spiteful, malevolent, but weak, and dangerous only in the dark. The negro's deity is more powerful, but not less hateful; invisible, indeed, but subject to pain, mortal like himself, and liable to be made the slave of man by enchantment. The deities of the South Sea Islanders are, some good, some evil; but, on the whole, more is to be feared from the latter than to be hoped from the former. They fashioned the land, but are not truly creators, for earth and water existed before them. They do not punish the evil, nor reward the good. They watch over the affairs of men; but if, on the one hand, witchcraft has no power over them, neither, on the other, can prayer influence them; they require to share the crops or the booty of their worshippers. . . . It appears, then, that every increase in Science—that is, in positive and ascertained knowledge—brings with it an elevation of religion. Nor is this progress confined to the lower races. Even within the last century Science has purified the religion of Western Europe by rooting out the dark belief in witchcraft, which led to thousands of executions, and hung like a black pall over the Christianity of the Middle Ages."

Other chapters of the work are devoted to the art and ornaments of savages, their marriage rites, and ideas of relationship, their morals, languages, laws. The Author considers, finally, that he has conclusively proved—"1. That existing savages are not the descendants of civilised ancestors. 2. That the primitive condition of man was one of utter barbarism. 3. That from this condition several races have independently raised themselves."

The Protoplasmic Theory of Life. By JOHN DRYSDALE, M.D. London: Baillière, Tindall, and Cox. 1874.

THE object of this work is to prove "that every action properly called vital, throughout the vegetable and animal kingdoms, results solely from the changes occurring in a structureless, semifluid, nitrogenous matter, now called Protoplasm." It lays claim to no originality, and the subject-matter is expanded from a lecture delivered before the Microscopical Society of Liverpool during the present year. The Author is evidently well acquainted with his subject; he is well acquainted with the literature of it, and quotes a multitude of men, from Schleiden, Schwann, and Leydig, to Beale, Herbert Spencer, and Huxley. He sums up the whole variety of opinion on the subject in the following opening sentences, which divide them into—" 1. Those which require the addition to ordinary matter of an immaterial or spiritual essence, substance, or power, general or local, whose presence is the efficient cause of life.; and 2. Those which attribute the phenomena of life solely to the mode of combination of the ordinary material elements of which the organism is composed, without the addition of any such immaterial essence, power, or force)." Up to the year 1835 the old idea of a *vital fluid* was in some form or other dominant; in that year the *coup de grace* was given to the theory by Dr. Fletcher, of Edinburgh, who, in his "Rudiments of Physiology," discussed the merits of the theory in a perfectly calm and judicial manner, and decided against it. The protoplasmic theory was proposed so recently as 1860, by Dr. Lionel Beale, who describes fundamental germinal matter, or bioplasm, as "always transparent and colourless, and, as far as can be ascertained by examination with the highest powers, perfectly structureless, and it exhibits these characteristics at every period of its existence. . . . Nor can any difference be discerned between the germinal matter of the lowest, simplest, epithelial scale of man's organism, and that from which the nerve-cells of his brain are to be evolved." Then follow commentaries on Beale's nerve theory and muscle theory; then various definitions of life, among which we may notice De Blainville's "Life is the two-fold internal movements of composition and re-composition ,at once general and continuous."

The concluding chapter treats of Materialism. The Author shows how frequently the term is misunderstood or wrongly defined, and shows, further, that religion and science need not be at war. The facts of the theory of protoplasm are clearly set forth in this very readable work, and the non-scientific man may by its help make himself well acquainted with one of the most prominent physiological theories now before the world.

The Elements of Embryology. By MICHAEL FOSTER, M.A., M.D., F.R.S., Fellow of and Prælector in Physiology in Trinity College, Cambridge, and FRANCIS M. BALFOUR, B.A., Fellow of Trinity College, Cambridge. London: Macmillan and Co. 1874.

THE Cambridge Physiological Laboratory is already beginning to bring forth good work, and it is satisfactory to see the name of a young student side by side, as joint author, with that of the able Prælector in Physiology. A few years ago physiological laboratories were almost unknown in this country: the work before us promises a splendid proof of what may be done in them, and of the value of the results to be obtained by a term or two of patient work. This book is only the first part of a systematic introduction to the whole subject of Embryology, and since the chick is not only "of all embryos the best to begin with," and also the most easy to obtain, and in many respects to manipulate, the Authors have taken it, from the new-laid egg to the chicken, as their starting-point. And they express hope, at the commencement, that the student will not content himself with merely reading the book, but that he will dissect for himself, in which he will be aided by many practical instructions, and by some beautiful and original drawings. The work is divided into nine chapters, which treat of the structure of a hen's egg, of incubation, of the changes which take place during respectively the first, second, third, fourth, fifth, sixth, and so on to the end of incubation; a final chapter on the development of the skull; and an Appendix, containing practical instructions for studying these various changes. Of the woodcuts we may specially mention Fig. 1, which shows a section of an unincubated fowl's egg; Fig. 13, showing the circulation of the yolk-sac at the end of the third day of incubation; and Fig. 71, showing the embryonic skull of a fowl during the second week of incubation. The formation of each particular organ—eye, ear, heart, &c.—is traced and illustrated in a most masterly manner. The book will be a great addition to our study of physiology. Although not suitable for school use, we have no doubt that the work will soon be adopted wherever examinations in physiology are held, and by those colleges which require physiology as one of the subjects of their Science scholarship examinations.

An Elementary Exposition of the Doctrine of Energy. By D. D. Heath, M.A., formerly Fellow of Trinity College, Cambridge. London: Longmans, Green, and Co. 1874.

This is a capital and comprehensive little book. It embodies a series of lectures given in 1872 to the sixth form of the Surrey County School. Mr. Balfour Stewart has an "Energy Class" at Owens College, and in process of time we shall have this great and dominant doctrine taught in all our larger schools. The sooner that time arrives the better, for we have already a good text-book in the work before us. It might, however, with advantage be somewhat subdivided, and the addition of a table of contents and of an index would enhance its value for purposes of reference. The Author gives at the outset a series of typical changes:— 1. A head of water confined by a sluice gate. 2. The sluice being opened the water falls, gaining velocity as it loses height. 3. The falling water encounters a water-wheel, and gives up to it some of its motion, the wheel in its turn giving motion to a system of machinery. 4. Heat is produced both by the clash of the falling water and by the friction of the bearing parts of the machinery, or millstones. 5. The millstones, working in a vessel of water, raise it to the boiling-point. 6. But no higher, for the water then passes into steam. 7. The steam so generated may work a steam-engine. 8. Which may work an electrical machine. 9. By which we may produce, through the intervention of the spark, both light and heat. 10. Or may decompose water. 11. While, finally, the liberated oxygen and hydrogen may be re-composed, and the steam produced may work a pump and replenish the head of water in the original reservoir. "Our proposition then is," says the Author, "that we may represent numerically—(1) the power to produce changes which the water in the reservoir has in virtue of its quantity, and its height above the mill-wheel; (2) how much of the power due to motion the water gains as it loses height in falling; (3) how much in motion, and how much in heat it gives to itself and to the mill-wheel when it clashes on it; (4) how much heat goes to turn water into steam; (5) what the steam loses in working the steam-engine; (6) how much the steam-engine is retarded by working the electric machine, and producing heat, and decomposing the water." Then comes a section on fundamental units; mass and force, velocity, &c.; then the second and third laws of motion; the measurement of heat; and the measurement of work done. The second section treats of dynamical energy; the third of thermal and other energies,—electrical, chemical, and what he calls "animal energy," and the energy of vegetation; while a short section at the end treats of molecular theories. The work is very sound and clearly constructed, and we recommend it to the student as a very useful book on a very important subject.

The Elements of the Psychology of Cognition. By ROBERT JARDINE, B.D., D.Sc., Principal of the General Assembly's College, Calcutta, and Fellow of the University of Calcutta. London: Macmillan and Co. 1874.

THIS work is designed for the use of students who are beginning the study of philosophy, and its object is to serve as an introduction to the psychology of the intellectual part of the human mind. After an introductory chapter, we have chapters on the acquisition of presentative knowledge, on the theories of perception, on representation, and a final chapter on the elaboration of knowledge. In the latter some useful matter concerning induction and deduction may be found:—"Induction proper has for its basis an observed relation of phenomena, or the elements of phenomena, in a greater or less number of cases; and the inference asserts what will take place in all similar cases. Inductive inference affirms, regarding all instances of a particular kind, what is observed to be true of a certain number of instances of that kind." Then as to deduction, "here the mind passes in reasoning from the general or universal to the particular." Thus the chemist, having discovered the general laws of chemical combination, can predict the result in particular cases; and the naturalist, having ascertained the universal characteristics of some species, may with certainty expect to find these characteristics in any new specimen of this species. A short summary is given at the end. The matter is clearly stated, and the book will be found useful to all those students who intend to take up that vast and not too profitable study of philosophy.

Transits of Venus. A Popular Account of Past and Coming Transits, from the first observed by Horrocks, A.D. 1639, to the Transit of A.D. 2012. By RICHARD A. PROCTOR, B.A. With 20 Plates and 37 Woodcuts. London: Longmans, Green, and Co. 1874.

WE have before us another of those carefully written and beautifully illustrated books which Mr. Proctor, from time to time, provides for the purpose of popularising more or less abstruse science. This work can hardly hope to appeal to so large a section of the public as his "Sun," "Moon," &c., for the subject is one that rather concerns the astronomer than the general public; the objects at issue do not bear so directly upon everyday life; and if a man has never seen Venus, and has no chance of ever seeing a transit, and does not easily comprehend how his race can be benefitted by a knowledge of an exacter determination of the sun's distance (which need not have a greater error than 100,000 miles), then we say he is not likely to trouble himself about the subject at all. But if any book is likely to create an interest on the subject, and to make it popular, that book is surely Mr. Proctor's.

Economic Geology, or Geology in its Relations to the Arts and Manufactures. By DAVID PAGE, LL.D., F.G.S., &c. Edinburgh and London: W. Blackwood and Sons.

As the Author remarks in his Preface, "There are works on agricultural geology, on building and decorative, on mortars and cements, on coal-mining, on veins and lodes, and on ores and metallurgy, but there is no general treatise on geology in its numerous relations to the arts and manufactures." This deficiency he has therefore endeavoured to supply, and has produced a work which we feel convinced will be widely appreciated. The purpose of this treatise being purely practical, those interesting theoretical questions in which Geology is so rich are here passed over. For a history of the life of the primæval world, for the causes of the striking phenomena it presents, for palæontological research, the reader must look elsewhere. Dr. Page, we must add, has, in his former publications, proved his ability to deal with such subjects. After an Introduction, showing the importance of Economic or applied Geology to the farmer, the land agent, the builder, the engineer, the miner, as well as to most manufacturers, the Author gives a brief account of the "rocky crust" of the earth, of stratified and unstratified rocks, their relative positions, texture, chemical composition, and chronological arrangement. He then shows, in succession, the bearing of geology upon agriculture, upon the valuation of land, upon architecture, engineering (including road and railway making, the construction of canals and docks, and the supply of water), upon mining, the production of heat and light, the fictile arts. He then treats of grinding, whetting, and polishing materials, of fire-resisting substances, of pigments, dyes, and detergents, of salts and saline earths, of mineral and thermal springs, of mineral medicines, of gems and precious stones, of metals and metallic ores. A summary at the end shows the valuable products likely to be found in each geological formation.

We need scarcely say that to deal exhaustively with all the subjects here enumerated would require not a volume, but an encyclopædia, and that of the bulkiest. Still whatever could be done within the compass of some three hundred closely-printed pages has been done. It would be difficult, indeed, to compress a larger amount of sound useful matter within the same limits.

We may, however, venture to point out that in the chapter on dyes tin is omitted amongst the bodies mentioned as mordants. Nor do we think it strictly accurate to pronounce the coal-tar colours of inorganic origin.

The work is illustrated with engravings, and accompanied with a geological map of the British Islands, placed, for better preservation, in a pocket within the cover.

We cannot more fitly conclude this brief notice than by quoting the following excellent advice:—"Every surface efflorescence, however insignificant, every trickle of styptic water (and every issue of water should be tested by the field geologist), every mealy disintegration of a rock, and even the presence of such plants as affect saline soils, should all be duly noted as indications of the mineral treasures below."

CORRESPONDENCE.

RESPECTING THE PHENOMENA CALLED SPIRITUAL.

By Charles Francis Keary.

Sir,—You have the satisfaction of thinking that you, more than any one else, have been instrumental in directing public attention to the "phenomena called Spiritualism," and that a strong and growing interest with many thoughtful people—and with not a few of considerable scientific attainment—dates from the publication of your first "enquiry" in 1870. This arose, no doubt, in great measure from the fact that you were the first scientific man who had devoted much labour to these investigations, and had made the result of them known; but partly also, I conceive, from a general impression in the public mind that you had not received very fair or decorous treatment either from the Royal Society or from the writer of a well-known article in the "Quarterly Review."

Speaking of the influence which the growing practice of publicly reading the Bible had in promoting the spread of Protestantism, Hallam makes the acute observation that this influence was largely due to the opposition which the practice met with from the Catholic Church, and to the fact that people were thus led to interpret the Scriptures "with that sort of prejudice which a jury feels in considering evidence that one party in a case has attempted to suppress; a danger," he goes on to remark, "which those who wish to restrain the course of free discussion, without any sure means of success, will in all ages do well to reflect upon." This consideration should alone be sufficient to determine us to give a fair hearing to the new science, or philosophy, or religion, or whatever it may be, which is comprised under the name of Spiritualism.

It may well be to this very prejudice of which Hallam speaks that we owe, to some extent, the activity of the public mind on this subject, and the decided advance which in the last few years the facts of Spiritualism have made towards—however far they may yet be from—a recognition. We must not therefore allow ourselves to be much influenced by a specious argument drawn from this fact, as well as from the greater candour of our own days, which is often employed in favour of other beliefs or disbeliefs beside the one in question. Because some things of a nature not altogether dissimilar from Spiritualism, which were once scoffed at, are now admitted by scientific men,—therefore, it is said, Science is sure, sooner or later, to give a place to this new theory. The same argument is often employed in religious questions, where it is contended that, because the upholders of religion have made many admissions and yielded many points once only maintained by sceptics, we are all inevitably drifting towards a complete scepticism at last. The argument is too foolish a one to be employed save in the heat of theological controversy, and I have wondered to find it so often even there. It only requires to be pushed one step farther to become a *reductio ad absurdum*. Because the representatives of Science or Religion, at any particular epoch, have almost as often as not been opposed to new discoveries, therefore there is no truth in either Science or Religion. Because, for instance, Harvey's discovery was scouted by the Medical Science of his day, therefore the pretender of ours who has found out that the earth is flat, or that the moon does not turn upon her axis, is as likely to be right as the Astronomer Royal. The fact, then, that Sir George Lewis, writing in 1849, spoke of Mesmerism or Electro-Biology as a theory which had been quite long enough before the world to have established its truth if there had been any in it, while the very facts of Electro-Biology are by many looked to for a probable explanation of Spiritualism, should not have more influence with us than to prevent our being turned from a candid and thoughtful examination of the subject by the passing effect of mere prejudice or fashion.

It is necessary to make this remark, because the more enthusiastic spiritualists seem now to be sustained by a premature confidence that such prejudice and fashion are the only obstacles which they have to overcome. A cause which is numerically weak has some advantage when its opponents think that the weakest arguments against it will suffice. Mere laziness, rather than want of capacity, must be blamed for the silly things which are constantly being said to disprove the truth of Spiritualism; but its more thoughtful supporters will do well to consider carefully if any valid arguments can be produced against them. It is with the object of inviting such consideration, Sir, that I have written this letter, which must be looked upon as an attempt—however unsuccessful—to put the question in the light in which it is likely to be regarded when the effect of prejudice has worn away. In the first place, then, I think it must be at once conceded that the present popular explanation of Spiritualism cannot be sustained. This popular explanation we know is this:—All the phenomena are nothing but a happy mixture of cheating and delusion. All those—scientific men and others—who have tried experiments upon these facts are deceived, just in the same way in which the writer or "the general reader" would be deceived by Messrs. Maskelyne and Cooke, only with this difference—that we should know we had been tricked, and they do not. This general opinion, I think, must change. To suppose that one man after another should enter upon the examination of these phenomena with a firm determination to expose the deception, and a perfect confidence of being able to do so, yet should end at last by being the victim of common trickery; to find, too, that many of these enquirers were well trained in the habits of scientific experiment,—this gives too rude a shock to our belief either in the value of human testimony or in the power of human skill in detecting falsehood and discovering truth. For on the hypothesis of mere trickery, of these three conclusions one:—Either all those enquirers who are ready to vouch for the truth of some of the facts of Spiritualism were—excuse the discourtesy for the sake of the argument—deliberate liars; or else they were less capable of investigating these facts than we—the general public—are; or, finally, if we had investigated them we should have been likewise deceived. Now I think, without any excessive modesty, we may put aside the second hypothesis. The dilemma in which we stand, therefore, is this:—Either concurrent human testimony of the highest seeming character is valueless, for all those who give it may be deliberate liars; or else we only preserve ourselves from error by not investigating,—in other words, the use of enquiry and reason is not a method of discovering truth. It is difficult to say which of these two conclusions is the most untenable.

Must we, then, admit the facts, and endeavour to discover some natural law which shall account for them? If these facts had not been of a nature different from those which rewarded your earlier enquiries, it would seem as if they might be admitted on an hypothesis which would give no violent shock to our present World-Theory. I need hardly say I allude to the hypothesis which you yourself started under the name of Psychic Force. Science is disposed to admit the phenomena to which has been given the premature and rather meanless name of Electro-Biology. This series of phenomena includes the action of one mind or brain upon another, and through that other brain upon the muscles of the body to which it belongs. The Psychic Force theory extends this notion to the affecting of outward material objects by the same brain power. In the ordinary experience of life we know of but one way of affecting another brain,—by the transmission of ideas through the recognised channel of speech,—just as we know of but one way in which the brain can affect inanimate matter,—that is, by the movement of the muscles of the body. To suppose a new exercise of this brain power in the second case is scarcely more difficult than to grant it in the first. Yet this is all the Psychic Force theory demands. As in Electro-

Biology, it says, we see a new brain-power acting not through the ordinary means of speech, but directly from brain to brain, so in this case we see such a new power acting not only upon the muscles or nerve-vessels of the body, but upon external matter beyond the precise region of the body.

Thus can the theory be stated so as to seem not to make very large demands upon our tolerance; and though it must be admitted that the otiose assent which is at present given to Electro-Biology arises chiefly from our ignorance of—or the absence of—exact mental laws, and our comparative indifference to such as we do know, nevertheless some force which may be compared to Electricity proceeding from the brain, under as yet unascertained circumstances, is certainly conceivable. But it is obvious that the readiness with which we assent to such a theorem must depend upon—must, in fact, vary inversely with—the degree in which these new phenomena modify our former experience. When it is only a question of moving a plank or a human body a little way in the air, we might accept it; but if asked to believe that this Psychic Force had the power of attracting a fresh comet (for this is not a radiating force, but can, at will apparently, be made to act in any particular direction and on any particular object), we should certainly refuse. This may be illogical of us, seeing that, if there is a new force discovered or come into being *for us*, it is as likely to be a great one as a little one; but we should have this comfort—that it could not be long before we should be obliged to believe in this new force, were it so powerful as it was said to be. Now, your later discoveries seem to point out this Psychic Force as the most remarkable in Nature. It apparently has the power of changing the very character of matter. Thus you record the fact of a bell having been carried through a locked door without any subsequent change in the nature of either of these material substances. The Psychic Force, then, must be accredited with the power of suddenly changing entirely the arrangement of the atoms composing bell or door, or both, and then as suddenly making them return to the *status quo*. It must be confessed that nothing can be conceived more contrary to all our past experience of matter and of force than this. All other experience teaches us that things are kept in their places by a balance of opposing forces. During the sudden rarefaction of the atoms of the bell or door, it cannot but have been that some of them came under the influence of quite new attractions, and passed into an entirely new state. How, then, can we conceive a force which should exert on each atom such a nicely-adjusted action as to bring it back exactly *in statum quo*, and no more? Is it not clear that such a force differs, *toto cœlo*, from all others in Nature, with their "equal and contrary reactions," as far as we have yet gone?

Other instances might be cited if necessary; but this alone is, I conceive, sufficient to show that these facts cannot be included under the head of "natural" phenomena. For it will be most germane to our present purpose, as well as most agreeable with the facts of Nature, if we draw a distinction between "natural" and "spiritual" at that point which divides *us* from external nature, or mind from matter. For I hold it to be a mere misnomer to speak of the will as a "natural" force in the sense of coming under the analysis of Science. Science, indeed, rather professes to disbelieve in the existence of this force than to correlate it with the forces of Nature; but until any one will prophecy what a man is *going* to think or do, and not only safely after the event, I shall continue to believe in it. Yet it certainly lies entirely outside the view of Science. It has no discovered correlation with "natural" forces. Science has simply nothing to say to it whatever. A body falling from rest to the earth will reach it in a time dependent only on the space through which it falls,—but not if I catch it and hold it in my hand during a time dependent only on my own will. We are so used to make these tacit exceptions that we scarcely realise that the forces of Nature do not act here with the same regularity as if there were no *wilful* beings upon our planet.

Keeping ourselves, however, within the limit of a purely scientific method, we observe that though this force Will has the power of directing certain internal forces of the body, yet it never acts upon matter but by the use of natural force. The expenditure of a certain amount of heat is the equivalent of the muscular action by which I sustain the falling body; nor have we any experience of this force of will acting upon matter directly, without the agency of known and measurable forces.

Our spiritual theory, therefore,—though, as we have used the word, it makes no assumption of the agency of disembodied spirits,—will not aid us much here, because we have already agreed that it would require an almost inconceivable amount of evidence to make us believe in the existence of a force which could make a bell pass through a door without permanently altering the condition of either. If, therefore, we are to accept the spiritual explanation of such phenomena as these, it must be as instances of this mental force acting directly on matter in a way of which we have at present no experience whatever. So that should by any chance this "spiritual" theory be the true one—and I do not say positively it is not—it would seem that Science is very unlikely to gain much by these investigations. For she has never yet, I venture to think, gained anything by the study of Psychology: I mean that, for all practical purposes, the laws of Mind are as distinct from the laws of Matter as ever they were.

Before, however, Science abandons the field of these phenomena as quite beyond her sphere, she is sure to rest for a long time upon a different explanation of these facts, which, though it does not bring them within the domain of Science, takes away their importance by robbing them of their external reality.

The series of phenomena called Electro-Biology seem to be chiefly connected together by the effect which can be produced upon the brain of the patient or patients by some unknown force generally believed to be at the command of some person. This effect includes the production of all sorts of curious delusions, and though these delusions have in no respect—as far as has been ascertained—been confounded, after the event, with the experience of common life, yet, as we know so very little of our own mental constitution, there is no reason to feel sure that no force is capable of producing delusions which would be so confounded. The real difference between our ordinary thoughts and those of dreams, trances, mesmerism, &c., is, it is to be noticed, the break in their continuity. In dreams these breaks occur not only at the moment of waking, but constantly throughout the whole succession of ideas. Thus, in dreams we never walk upstairs, or from one place to another,—I think I am speaking advisedly when I say *never*,—but we suddenly *find ourselves* arrived at the fresh place; and the same breaks may be noticed all through the dream. On the other hand, illusions produced by illness occur to perfectly sane people, without break of continuity; though in these cases they are generally but of short duration. The Electro-Biological explanation of Spiritualism would suppose these delusions to be as long as those produced in mesmerism, but also to occur without break in the continuity of ideas. The fact of permanent witnesses remaining to testify to the occurrence will not in this case serve, because a perfectly natural occurrence may happen, and the extraordinary explanation of it may be the result of delusion. Thus it would, I believe, be quite possible to mesmerise a person, then to jump upon a chair and write upon the ceiling, making the person believe all the time that you were floating in the air.* Nor can we in this case apply the tests which would have served if we adopted either the Psychic Force Theory or the Spiritual Theory,—in the larger sense in which I have used this latter word,—the test, namely, of knowledge being communicated to one person in the room, such as could have been known to him alone; because all this, the whole idea of this knowledge or of any communication, may have been part of the delusion. Take, for instance, the case in which you received communications by means of the Morse alphabet. The whole idea, from beginning to end, may, according to this theory, be a delusion; all that originated with the Medium (or blind Biological Force, possibly) being the idea put into your mind of having received a special message, the rest being left to a certain unconscious constructive power which the mind has, as we know from dreams,* &c.

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* The same explanation would, I think, apply—*mutatis mutandis*—to the spirit photographs. I only suppose cheating on the part of some person, to make the argument *clearer*—it does not make it any *stronger*.

I can imagine that such an explanation may be to you and your fellow-investigators a distasteful one, though not, so far as I can see, altogether reasonably so; nor am I ignorant that some facts are conceivable which would make this theory untenable. But I do not think at present that these facts, if they exist, have been produced; and until they are produced I feel sure that this is the explanation on which Science will ultimately rest,—being, indeed, the most reasonable from a purely scientific stand-point, though not on that account necessarily the *true* one. For Science, I hold, is not the same thing as Philosophy, and does not embrace the whole World-Theory of any man. The consideration of the subject from a philosophical stand-point would be somewhat different, and is not at present within my scope. The difference may, however, be hinted in the following way:—If the Electro-Biological Theory is true, it would seem very probable that these *delusions* are the result of certain undiscovered physical conditions of the mind, rather than the conscious action of one person's will upon others. Now, suppose these conditions liable to affect all, so that—without any break in the continuity of our ideas—we were all liable, under certain circumstances, to see bells pass through doors. What would be the difference in this case from the fact that bells could, under certain circumstances, pass through doors? An attentive consideration of this question will show us that if these "certain circumstances" were the same for all, there would be no difference. But that they are the same for all those who simultaneously are affected by the delusion our theory presupposes. If therefore the delusion became universal, it would cease to be a delusion and become a *fact*.—I am, &c.,

CHARLES FRANCIS KEARY.

British Museum.

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* As, for instance, when we hear a noise in sleep we almost always make it fit in with our dreams.

PROGRESS IN SCIENCE.

MINING.

Some researches on the geology of the mining districts of Cornwall, with special reference to the formation of mineral veins, have recently been submitted to the Geological Society by Mr. John A. Phillips, whose extended study of the microscopic structure of Cornish rocks and veinstones, coupled with his chemical examination of the rocks, must needs give peculiar weight to the conclusions which he has reached. He believes that the vein-fissures of the tin- and copper-bearing lodes of Cornwall were the result of forces acting subsequently to the solidification of the elvans, but in the same general direction as those which gave rise to the eruption of this rock. The fissures produced by these forces afterwards became filled with minerals, which were deposited by chemical action from waters circulating through them: these waters were probably, in some cases, at a high temperature, through contact with highly-heated rocks at great depths, but in other cases the temperature of the circulating waters appears to have been but moderate, and the action consequently but slow. It seems impossible to determine to what extent these deposits were produced by waters rising from below, or how far they were influenced by lateral percolation, but the latter action has probably been important. Contact-deposits and stock-works have been formed by analogous chemical action, set up either in fissures resulting from the junction of dissimilar rocks, or in fractures produced during the upheaval of partially-consolidated eruptive masses. England, unlike Germany, has so few experienced miners who are at once good chemists, mineralogists, and geologists, that such a paper from a man like Mr. Phillips is certainly a rarity, and deserves accordingly to be thoughtfully studied by the scientific metal-miner.

A description of the deposits of phosphorite in North Wales has recently been laid before the Geological Society, by Mr. D. C. Davies, of Oswestry. The phosphorite bed varies from 10 to 15 inches in thickness, and occurs at the top of the Bala limestone over a considerable area, having been found in various localities from Llanfyllin to the hills north and west of Dinas Mawddy. The bed appears to consist of concretions of various sizes, embedded in a black matrix. It is notable that the nodules are coated externally with a thin black film of some graphite-like mineral. The concretions contain about 64 per cent of tribasic phosphate of lime, whilst the average proportion in the entire bed—including nodules and matrix—is about 46 per cent, This deposit has been worked at certain points, and promises to become an important object of exploration.

It is reported that a very large deposit of phosphate of lime has been discovered in the island of Amba, in the Caribbean Sea.

Some interesting relics of ancient mining have been recently brought to notice by Prof. Boyd Dawkins, who has discovered—in the old workings near Alderley Edge, in Cheshire—a large number of rude stone hammers, similar to those which have been found in association with old copper-mines at Lake Superior, in Spain, Portugal, and elsewhere. It is difficult to fix the age in which these rude mauls and picks were used in Cheshire, but it is probable that they go back beyond the range of our history, and may therefore be fairly called "pre-historic." They were found on Lord Stanley's property, where the copper-bearing sandstones of the Trias are now worked by the wet-way, It appears, however, that some old slags discovered in the neighbourhood were not found to contain any copper, and hence it is probable that other metals may also have been worked here at a comparatively early date.

As mining geologists must take a good deal of interest in the deep boring at Netherfield, generally known as the Sub-Wealden Exploration, we may mention that the Committee—after having spent many months in fruitless attempts to recover the boring-tools, which had unfortunately fallen to the bottom of the deep hole—came to the bold conclusion that the old bore should be abandoned, and that a new one should be commenced by the Diamond Rock-Boring Company. The original boring had reached a depth of upwards of 1000 feet, and was advancing well in the Oxford Clay, but it was found on survey to be slightly out of the perpendicular, and the recovery of the lost tools seemed altogether hopeless. Under these unfortunate circumstances the Diamond Company generously undertook to put down a 1000-foot bore, with lining, at the cost of only £600. After a good deal of discussion as to the selection of the new site, it was agreed that the second boring should be close to the first, and accordingly the Netherfield bore-hole No. 2 was commenced on February 11th. A 6-inch crown is employed, and cores of almost this size are now being extracted.

Since the Sub-Wealden boring has attained to a considerable depth, attention has been directed to other deep borings, and to the difficulties experienced in such undertakings. Perhaps the most interesting of these deep bore-holes is that which was sunk a few years ago at Sperenberg, about 25 miles south of Berlin. The rock at the surface was gypsum, but this passed down into an enormously thick deposit of rock-salt. The boring was commenced on the 25th of April, 1867, and on reaching a depth of 984½ feet (956 Prussian feet), in July, 1868, it was stopped, in order that steam-power might be substituted for hand-labour. After the arrangements for introducing steam were completed, the boring was resumed, and continued uninterruptedly from January, 1869, to September, 1871, when the hole had reached the extraordinary depth of 4172½ feet (4051·6 Prussian feet).

The "Colliery Guardian," in its issue for the 1st of January, published some interesting statistical tables, giving a complete record of the lives lost by colliery accidents during the thirteen years which ended on December 31st, 1873. Without analysing these figures, we may state that the grand totals show that the aggregate number of deaths during this period amounted to 13,756. Of this number, 2790, or only little more than one-fifth, were due to explosions. The improved condition of our mines is seen by the fact that in 1861 one life was sacrificed for every 91,240 tons of coal raised, whereas in 1873 one life was lost for every 133,677 tons of fuel.

It would be wrong to conclude this Chronicle of Mining without mentioning that the Murchison Medal has this year been awarded, by the Geological Society, to Mr. W. Jory Henwood, of Penzance, as a well-merited recognition of his labours in mining-geology. Mr. Henwood's valuable observations on mines in almost every part of the globe will be found in the fifth and eighth volumes of the "Transactions of the Geological Society of Cornwall," two volumes of which are entirely devoted to his papers, and are an enduring proof of the success with which he has brought a sound scientific knowledge to bear upon the miner's art.

METALLURGY.

Remarkably little progress appears to have been made during the past quarter, either in the development of any branch of our metal industries or in the publication of papers bearing upon the scientific aspect of metallurgy. Our report is consequently somewhat meagre.

With respect to the metallurgy of iron, we may mention that certain improvements in puddling have been patented by Sir J. G. Alleyne, of the Butterly Iron Works. He provides his furnaces with four regenerative chambers; two for gas, and two for air. The rabble used consists of iron bars or tubes, bent to a loop or crank form, and made to revolve on a horizontal axis, so that the loop or crank can sweep through the metal in the basin of the furnace.

An improved rotary puddling furnace has been devised by M. Ehrenwerth. This furnace has a revolving sole, moved by a system of geared wheels. As the charge of pig-iron melts, the sole is put in motion, and the metal is worked by means of rabbles furnished with peels set obliquely. The rabbles rest on cones, and may be moved by either manual labour or steam-power.

Some improvements in the manufacture of iron and steel have been introduced by Mr. Willans. During the re-heating of the iron he injects into the furnace oil, creosote, carbonic oxide, or other substances containing carbon or hydrogen, in order to prevent the oxidation of the metal. Steel tubes may be made by employing soft steel, which is easy to draw, and then annealing this in the presence of deoxidising agents.

A process for tempering steel, invented by MM. Garnaut and Seigfield, has been acquired, it is reported, by the United States Government. It consists in heating the steel to cherry-redness, and then sprinkling it with common salt. At a later stage of the working a mixture is employed, containing chloride of sodium, sulphate of copper, sal-ammoniac, carbonate of soda, and saltpetre. Finally, the steel—having been brought to a cherry-red heat—is plunged into a bath containing alum, carbonate of soda, sulphate of copper, saltpetre, and chloride of sodium.

Mr. J. M. Oubridge, of Middlesbro', has recently read, before the Cleveland Institution of Engineers, a paper "On the Construction of Foundries." He sketches the way in which he believes a foundry should be constructed, and enters into technical details suggested by his own experience. The paper has been reproduced in the columns of "Iron."

For many years past antimony ore has been worked and reduced in Borneo and of late years mercury has also been extracted. A description of the methods followed in mining and smelting these minerals has been laid before the Tyne Chemical Society, by Mr. T. Down, jun. The antimony mines are at Jambusan, about 30 miles from Sarawak. The ore—which is principally the sulphide, although native antimony and the oxide also occur—is found in veins running through limestone, and in derived boulders. A charge of about 30 cwts. of ore is introduced into a specially-constructed reverberatory furnace, where it is liquated. The crude metal is run into cast-iron moulds, while the oxide which has been volatilised is collected in long flues. The cinnabar occurs disseminated through a rock, said to be a basalt, at Tegora, about 10 miles from Jambusan.

MINERALOGY,

In compliment to Hofrath Kopp, of Heidelberg, the name of *Koppite* has recently been given—by Prof. Knop, of Carlsruhe—to a mineral from the Kaisersthuhl, previously mistaken for pyrochlore. It is found to be a niobate of various metals, including calcium, cerium, lanthanium, didymium, potassium, sodium, &c.: a part of the oxygen appears to be replaced by fluorine. Koppite occurs, with apatite and magnoferrite, in the granular limestone of the Kaiserstuhl.

Some interesting researches on the composition of *Autunite*, with special reference to the condition in which the water exists, have been undertaken by Prof. A. H. Church, and the results published in the "Journal of the Chemical Society." Specimens recently raised in the neighbourhood of Redruth were subjected to examination, and compared with others from near Autun, in France. The powdered mineral submitted to dry air, confined over oil of vitriol, lost between 8 and 9 per cent of water, at the same time becoming fragile and losing its transparency. These changes seem to indicate that the water thus removed is not moisture accidentally present in the interstices of the substance, but water which is absolutely essential to the constitution of the mineral. Over oil of vitriol *in vacuo* a further loss of water occurs, reaching about 15 per cent. Prof. Church's analyses lead to the following formula for the unaltered crystals:—$(U_2O_3.CaO)P_2O_5.10H_2O$; and show that Autunite dried *in vacuo* has only two molecules of water instead of ten. *Torbnerite*, a phosphate of copper and uranium, closely allied to Autunite, did not exhibit a similar behaviour when dried.

Prof. Weisbach, of Freiberg, has described a mineral brought by Herr Simon from Mancayan, in the island of Luzon, to which the name of *Luzonite* has been given. It occurs, associated with enargite, in veins of copper ore. Dr. C. Winkler's analysis shows that it is an arsenio-sulphide of copper, having essentially the same composition as enargite, which is consequently dimorphous.

Rauite is the name which has been bestowed upon a new zeolitic mineral from Brevig, in Norway, described by Herr Paykull. It appears to be closely related to Thomsonite, and to result from the alteration of Elæolite.

Some interesting pseudomorphs from the Tilley Fort Iron Mines, in Putnam Co., New York, have been described in the "American Journal of Science", by Prof. Dana. After sketching the geological structure of the district, he describes the several kinds of pseudomorphs; some consisting of serpentine, or of serpentine and dolomite, whilst others are composed of brucite, magnetite, pyrrhotite, &c.

Dr. J. Lawrence Smith has called attention to a curious association of essonite, or cinnamon-stone,—a variety of garnet,—and green idocrase, with datholite, in limestone, at Santa Clara, California. This is the first instance in which datholite has been observed in association with garnet and idocrase. Analyses are published of the several minerals here described.

By the same chemist we have a note on *Warwickite*, recently presented to the French Academy. This rare mineral is a boro-titanate of magnesia and iron, originally described by Shepard.

It may be interesting to the mineralogist to know that M. Radominski has succeeded in artificially producing the two rare minerals *monazite* and *xenotime*. The former is a phosphate of cerium, lanthanium, and didymium, whilst the latter is a phosphate of yttrium with the monazite metals.

Some notes by Mr. W. Skey, of New Zealand, contributed to the "Chemical News," are of much interest to the mineralogist, from their bearing upon the origin of *Torbanite*, or the celebrated Torbane Hill mineral. By allowing petroleum to filter through clay, he obtained a substance strikingly resembling the natural mineral, and he concludes that Torbanite is not a coal, but a chemical combination of an acid hydrocarbon with silicate of alumina.

Attention has recently been called by M. Daubrée to some highly interesting examples of the formation of metallic minerals within a comparatively recent period. M. Daubrée's observations on the minerals formed in the Roman works at the hot springs at Plombières are well known; but the present illustrations have been presented by the hot springs of Bourbonne-les-Bains, in the Department of the Haute Marne. It appears that during some recent excavations the bottom of the old Roman well was laid bare, exposing a bed of mud, in which a number of bronze, silver, and gold antiquities were discovered. This bed rested on a deposit of fragments of rock cemented into a brecciated mass by the crystallised metallic sulphides, Among these were found examples of copper-pyrites (sulphide of copper and iron), copper-glance or vitreous copper-ore (disulphide of copper), purple copper-ore or erubexite (sulphide of copper and iron), and fahlerz or tetrahedrite (sulphide of copper and antimony). It is true that most of these had been found in other localities, under somewhat similar circumstances, but the occurrence of the fahlerz in sharp tetrahedral crystals has not been previously recorded from any deposits of this character. Altogether the association is strikingly like that in some of our old copper-lodes, yet the deposit in question is certainly not older than sixteen hundred years. The bronze objects have been much attacked, but the silver has not been affected.

GEOLOGY.

Physical Geology.—The plant-bearing series of India has been shown by Mr. H. F. Blanford to range from early Permian to the latest Jurassic times, indicating that, with few and local exceptions, land and fresh-water conditions had prevailed uninterruptedly over its area during this long lapse of time, and perhaps even from an earlier period. In the early Permian there is evidence in the shape of boulder-beds and breccias underlying the lowest beds of the Talchir group of a prevalence of cold climate down to low latitudes in India, and as the observations of geologists in South Africa and Australia would seem to show in both hemispheres simultaneously. With the decrease of cold the author believed the Flora and Reptilian Fauna of Permian times were diffused to Africa, India, and perhaps Australia; or the Flora may have existed somewhat earlier in Australia, and have been diffused thence. The evidence he thought showed that during the Permian epoch India, South Africa, and Australia were connected by an Indo-oceanic continent, and that the first two remained so connected, with at the utmost some short intervals, up to the end of the Miocene period. During the latter part of the time this continent was also connected with Malayana. The position of the connecting land was said to be indicated by the range of coral reefs and banks that now exists between the Arabian Sea and West Africa. Up to the end of the Nummulitic epoch, except perhaps for short periods, no direct connection existed between India and Western Asia.

Mr. J. W. Judd has carried his investigations into the structure and age of Arthur's Seat, Edinburgh; he believes that in it we have the relics of a volcano, which was at first submarine, but gradually rose above the Carboniferous Sea, and was the product of a single and almost continuous series of eruptions.

Palæontology.—Messrs. Hopkinson and Lapworth have described forty-two species of Graptolites from the Arenig and Llandeilo rocks of North Wales. The Arenig rocks contain a number of species, which ally them to the Quebec group of Canada.

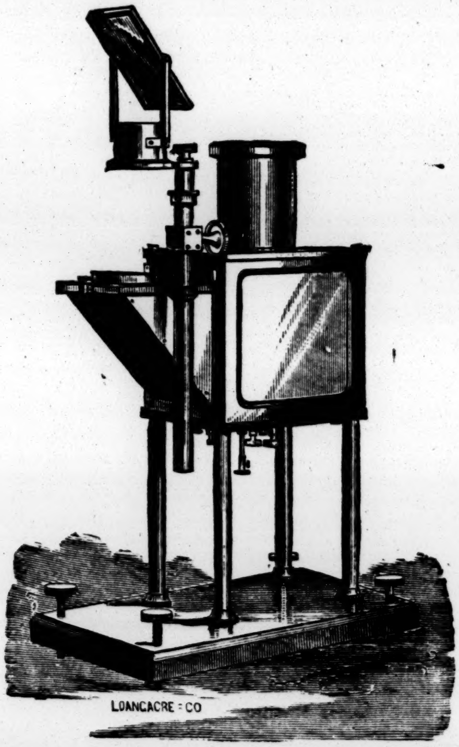
The publication of a series of decades, or numbers containing ten plates, illustrative of the palæontology of Victoria, has been commenced by Professor M'Coy. The first decade contains figures and descriptions of Graptolites, Marsupiata, Mollusca, Plants, and Star-fishes. Nearly all the species of Graptolites can be identified with species found in England or America. Extinct forms of Kangaroo and Wombat are described. Amongst the Mollusca, species of *Voluta* seem scarcely distinguishable from our Eocene forms. The plant-remains described include species of *Zamites* and *Lepidodendron*; the star-fishes belong to the family *Urasteridæ*.

Obituary.—By the death of Sir Charles Lyell, geological science has lost its greatest leader, one who, for a period of half-a-century, has been devoted to its advancement. He was born in 1797, and in 1819 was elected a Fellow of the Geological Society of London; he was President in 1835, and again in 1849. Sir Charles Lyell is deservedly designated the Historian of Geology; the amount of original research in the shape of detailed investigation which he performed has been exceeded by many of our distinguished geologists of the past and present; but the experience gained during travel, and the philosophical deductions he was enabled to draw in explaining the past history of the earth by the light of the present, in his "Principles of Geology," and in bringing together, in his "Elements" and "Manual," the known facts of geology, enabled him to influence the progress of the science more than any other man. The "Antiquity of Man, or early History of the Human Race," place all under a great debt of gratitude to the illustrious author. Although to the last he lost none of his interest in the progress of science, yet even in 1866, when he received the Wollaston Medal, he observed that every year he felt less able to keep pace with the ever-increasing rate at which geology is expanding, together with the numerous sciences which are so intimately connected with it.

Professor Marsh has recently returned from an expedition to the Rocky Mountains, where, south of the Black Hills, a deposit of Miocene age was found to be exceedingly rich in Mammalian remains. Nearly two tons of fossil bones were collected, including several species of gigantic *Brontotheridæ*. Professor Hayden has, however, lately expressed his opinion that the genus *Brontotherium* of Marsh is synonymous with the *Titanotherium* of Leidy, of which there are probably not more than two species.

PHYSICS.

Light.—At the late Industrial Exhibition of the Franklin Institute in Philadelphia, as also at the like Exhibition of the American Institute in New York, silver medals were awarded for the vertical lantern shown in the accompanying figure, which is manufactured by the instrument makers to the Stevens Institute of Technology, Hoboken, N.J. The cut shows the apparatus as adjusted to exhibit on the screen such objects as waves in water, cohesion figures, and the like. The light from the lime cylinder or electric arc passes through two large lenses, by which it is thrown in a parallel beam on an inclined mirror in the triangular box in front. By this it is reflected upwards, and is condensed by a large lens standing horizontally on top of the same triangular box, so as to pass into the objective above, and is then by the small upper mirror thrown on a screen. This upper mirror, with the objective, is carried by a bar provided with rackwork, by which an accurate adjustment for focus can be made. The objects rest directly on the plate holding the large horizontal condenser. When the apparatus is to be used for ordinary objects which can be held in a vertical position, a little screw in the front of the horizontal plate is taken out, and then the triangular box carrying the lower mirror is removed, allowing the horizontal plate to swing down into a vertical position, carrying with it the rackwork bar and objective into their proper positions.



Microscopy.—Mr. T. Charters White, at a recent meeting of the Quekett Microscopical Club, gave the results of his experience in maintaining small Marine Aquaria, for the purpose of supplying objects for a microscopical study. Almost any vessel capable of holding a quart or more of water may be made use of, although the tank preferred by Mr. White is the well known form constructed of slate and glass, divided into two portions by a sloping partition, on which the rockwork is arranged, the lower part being devoted to the storage of sea-water away from the influence of light, and consequently in a quiescent state; this arrangement renders the maintenance of the water in a healthy condition much easier than when the whole contents of the tank are freely exposed. To establish a new aquarium requires considerable care, and, owing to the neglect of a few necessary precautions, failures are common. The tank, with its rockwork, should be filled with *fresh* water, and all the soluble lime salts in the cement used in attaching the pieces of stone soaked out; this may take a week or two, and must on no account be hurried. Afterwards the sea-water, natural or artificial, may be introduced,* and some ponds of *Ulva* or other suitable seaweeds placed in the tank, the object being to promote the growth of minute algæ on the surface of the rockwork. A seaweed already growing, although attached to stone, cannot be trusted to maintain the necessary supply of oxygen, as it but rarely establishes itself and forms vigorous plants. The rock should consist of sandstone or mica schist, both having surfaces favourable to the growth of the minute vegetation so much needed, and suitable for the attachment of zoophytes and other microscopic animals. When a good crop of weed has established itself, which will be easily known by the copious evolution of air bubbles from the coated surface of the stone, animal life may be *cautiously* added, a few sea-anemones and serpulæ; but fish, crustaceæ, or any of the more active creatures must be carefully excluded from an aquarium devoted to the culture of minute animal and vegetable organisms. The microscopic stock finds its way into the tank by means of germs, which are sure to come with weed and rock, brought fresh from the sea. The *Foraminiferæ*, *Hydroida*, *Polyzoa*, and other minute forms, are not long in making their appearance, and supply the microscopist with an unending source of material for investigation. Microscopic slides should be placed in various parts of the aquarium, as minute growths developing on them can readily be removed for examination. Artificial aëration, which is carried on with such great success at the Crystal Palace and elsewhere, is hardly needed in small aquaria containing only a few small and inactive animals, but it can be obtained to a sufficient extent, by allowing water from a suitable source of supply to fall into the aquarium through a glass tube drawn out to a point; this small stream will carry with it a large quantity of air bubbles, and greatly aid in maintaining the purity of the water. Sea-water must on no account be allowed to come in contact with any portion of metal.

Mr. Ingpen, Secretary of the Quekett Microscopical Club, communicated some notes on "Personal Equation" as affecting microscopical observations. The term is a well known one in practical astronomy, and the same causes disturb microscopical results, although, in the latter case, they are not so well recognised. Mr. Ingpen called attention to the following points:—

I. *Mental* equation, as causing differences in interpretation, particularly with regard to test objects.

II. *Nervous* equation, as shown by varied sensibility to tremors, &c.

III. *Colour*. Difficulty in estimating colour, as noted in Admiral Smyth's "Sidereal Chromatics." Right and left eye often differ in this respect. Effect of yellow crystalline, referred to by Professor Liebrich in his lecture on "Turner and Mulready." Difference in visibility of violet end of spectrum, amounting, in some cases, to a slight fluorescence. Effect of bluish mist, caused by slight opacity of cornea upon estimation of the correction of objectives. Colour blindness often existing in a slight degree unsuspected, and difficult of detection.

IV. *Focal Equation*. Differences in effect of long and short sight upon cover correction, &c.; also upon depth of focus, and power of resolving surface markings. Differences in size of images formed by right and left eye, and consequent effect upon binocular vision. Want of accommodation and pseudoscopic vision, &c.

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* Where natural sea-water cannot readily be procured, the following artificial substitute from the formula of Mr. P. H. Gosse may be used:—

Chloride sodium (common salt)	3½ ounces.
Sulphate magnesia (Epsom salts)	¼ ounce.
Chloride magnesium	200 grains.
Chloride potassium	40 ,,
Water	1 gallon.

The iodides, bromides, and some lime salts found in minute proportion in sea-water are not included, but it is found that, when the aquarium has been established for a considerable time, these compounds may be detected.

V. *Form.* General tendency of the eye to show ultimate particles circular. Effect of square and triangular apertures. Effect of astigmatism upon form, particularly of lines and dots, as seen in different directions. Effects of diffraction upon points of light, &c. General considerations of the effects of unnoticed differences of vision, producing discrepancies often attributed to other causes.

ELECTRICITY.—The "Journal des Debats" notices a very ingenious application of electricity to voting in the National Assembly. "Before every Deputy two ivory buttons are placed, like the buttons of electric bells. If the Deputy wishes to vote 'Yes,' he presses the button on his right; if he wishes to vote 'No,' he presses the button on his left. The voter establishes by this means an electric communication, which is transmitted to an apparatus close to the President and his secretaries. Every time the electric current acts thus it opens the door to a ball, and the ball falls through a tube into the ballot box. The balls are made of glass or ivory, and are strictly identical in weight. The two ballot-boxes are then weighed, and the number of balls is indicated by the weight. Finally, by turning a handle, all the balls which have not been used are let out, and they give the number of members who have abstained, or were absent when the vote was taken. Nothing can be more simple. The inventor (M. Jacquin) has offered to set up his apparatus in the Versailles Assembly for the sum of 60,000 frs. Time is money." The "Debats" mentions also another plan invented by M. Martin, a well-known electrician. M. Martin's plan does away with the scales, which might not always be true. Accordingly, as the vote is black, a piece of coloured pasteboard appears instantaneously above a line bearing the name of the Deputy. Before each Deputy is a small box, supplied with two buttons. When he presses on one or the other, he discloses the piece of white or black card on the board. The sum total of the votes for either side is marked on a totalising board. The advantage of this system over that presented by M. Jacquin is, that it enables the President to see whether a Deputy has not voted because he abstained or because he was absent. A member can, by placing his hand on both buttons, vote at once "Yes" and "No," and be thus numbered among the abstainers.

TECHNOLOGY.—A recent number of the "Bulletin of the Industrial Society of Mulhouse" brings a very interesting report on "The New Dyes of Croissant and Bretonnière," from which we gather that a special committee has been investigating them in order to ascertain their value for the various purposes of dyeing and printing. At first the great durability of these colours engaged the attention of the members of the committee. Ink spots could, for instance, readily be removed by means of oxalic acid without the dyes themselves being changed in the least. The sunlight, as far as observations go, seems not to exert any influence on these colours; boiling soap-solution and oxalic acid are without effect, only chlorine or hypochlorites affect or rather destroy them.

In a paper on "Certain Properties of Weighted Black Silks" M. J. Persoz shows that weighting—which began with the modest aim of making up the loss sustained in ungumming—is now carried to the extent of 100, 200, and 300 per cent. This increase of weight is produced by treatment with salts of iron and astringents, salts of tin and cyanides. The bulk is augmented proportionably to the weight. As a matter of course, the chemical and physical properties of the silk thus treated are materially modified. What is sold as silk is, in fact, a mere agglomeration of heterogeneous matters, devoid of cohesion, held temporarily together by a small portion of silk. The elasticity and tenacity of the fibre are sensibly reduced. From being in its natural state one of the most permanent of organic bodies, and sparingly combustible, it burns like tinder if touched with flame. It is, moreover, liable to undergo spontaneous decomposition, and to absorb gases with the evolution of heat, which sometimes leads to actual combustion. The adulterated silk when burning scarcely gives off the characteristic odour of animal matter. It leaves an ash of oxide of iron exceeding 8 per cent.

ERRATA.—Page 145, line 18 from top, *for* "Iceland," *read* "Ireland." Page 148, line 4 from top, *for* "marked," *read* "masked." Page 155, line 20 from top, *for* "founded on a sound data," *read* "founded on sound data."